





M. B. Snyder. M. D.

PHYSICAL SKETCHES,

OR

OUT LINES OF CORRECTIVES,

APPLIED TO

Certain Modern Errours in Physick ;

BY ✓

JOHN B. DAVIDGE, A. M. M. D.

*Professor of the Institutes of Physick and of Anatomy in the
University of Maryland.*

Quantula sunt hominum opuscula exammen solum fatetur.

2 v. 5.

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DISTRICT OF MARYLAND, ss.

BE it remembered that on this Thirteenth day of December, in the Thirty-Eighth year of the Independence of the United States of America; John B. Davidge, of the said district, hath deposited in this Office, the Title of a Book, the right whereof he claims as Author; in the words and figures following, to wit—

§ SEAL. §

“Physical Sketches, or out lines of correctives, applied to certain modern Errours in Physick; by John B. Davidge, A. M. M. D. Professor of the Institutes of Physick and of Anatomy in the University of Maryland. Quantula sunt hominum opuscula examen solum fatetur.”

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PHILIP MOORE, *Clerk,*
of the District of Maryland.

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1872

Jan 1st ...
Feb 1st ...
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Nov 1st ...
Dec 1st ...

PREFACE.

IN the present volume the author takes some notice of the opinion of writers on the menstuous operation in the human female, and endeavours to show that little or nothing of correct science had enlightened the path of the philosopher until 1793, the period at which the author's sentiments were first given to the world.

However respectful the manner may be in which the author refers to Hunter, Denman, &c. it is very much of a fact that he received from the authorities alluded to but slender aid, their ideas being rather indeterminate and equivocal, by no means moulded into the form of a doctrine, or assuming the order of a principle. They appear to be rather an incidental conjecture than a digested result of a scientific view.

The author brings before the reader some of the many errors of writers, especially those of his own country, on the Yellow fever, not omitting to point out as he passes on, the facility with which even learning and character are influenced by the magick power of a name.

The gratuitous doctrines of Dr. Jones of London, on the Pathology of wounded Arteries, is

not passed by in silence ; nor does the writer forget the suppositious Aneurism of Philadelphia—that in which so much is said to suggest to a prudent understanding the advantage and propriety of always, with care, noting the marked distance between *opinion* and *fact*.

The author intends to pursue his plan of furnishing sketches or out lines of correctives on certain errours of modern works ; and although his talents may not be fully adequate to a successful management of the vast mass of materials provided to his hand, he has some hopes arising from his industry and application.

It is not to be understood that his hopes are very great of adding much to the fertility of the fair field of medical science, by directing through it many copious streams of knowledge from new and unexplored sources : he will have the measure of his expectations filled up, if he can succeed in giving a proper diversion to the inundating tide that has been for years setting in upon it. To act negatively is not at all times to act uselessly.—To be disabused of old errours is to be put into the way to acquire new truths.

DISSERTATIO PHYSIOLOGICA

DE

CAUSIS CATAMENIORUM

QUAM

ANNUENTE SUMMO NUMINE

ET AUCTORITATE DIGNISSIMI VICE-CANCELLARII

ARCHIBALDI DAVIDSON, S. S. T. P. P.

Et Collegii Glasg. Praefecti :

NEC NON

Amplissimi Senatus Academici consensu et noblissimae Facul-
tatis Medicæ decreto ;

PRO GRADU DOCTORIS

Summisque in medicina honoribus et privilegiis rite ac legitime consequendis,

IN COMITIIS

UNIVERSITATIS GLASGUENSIS,

Eruditorum examini subjecit

Joannes Beale Davidge, A. M.

Marylandiæ civis

SOCIET. AMER. PHYS. EDIN. SOC.

ET PRÆSES ANNUUS

Ad diem xxii Aprilis. hora locoque solitis GLASGUÆ.

M,DCC,XCIII.

MEDICIS

JACOBO, ET GULIELMO MURRAY

SUB QUORUM AUSPICIIS,

PRIMUM FUNDAMENTUM JECIT,

EXERCITANTIBUS MEDICAM ARTEM

DEBITA LAUDE

De Annapolis

SUMMÆ CHARITATIS PERTENUE SIGNUM,

HOC OPUSCULUM

DEDICAT

CONSECRATQUE

AUCTOR.

DE CAUSIS CATAMENIORUM.

INTER plurima, de re medicâ, argumenta, quod ex iis, hac in dissertatione, seligerem, quodque, optimo cum successu, tractare possem, diu et multum dubitavi. Omnibus vero rite et considerate perpensis, pauca de catameniis proponere mecum statui. Quibus ex causis oritur hic sanguinis fluxus, certo ætatis tempore, vel quid sit naturæ huic respondens consilium, in corpore fœminino, nemo adhuc, sive medicus, sive physiologus, pro certo dicere ausus est. Inter facultates aut functiones corporis humani nulla est de quâ difficilior ratio reddi potest. Multi quidem physiologi, tum veteres, tum recentiores, ad hanc rem explorandam, operam dederunt: ibi vero, me saltem iudice, labor eorum omnis effusus est, si quas in lucem ediderunt hodierni observationes excipiantur.

Nihil quidem, de hac re, novi, pauca tantum, quibus sententias Hunteri, Denmani, aliorumque medicorum stabilire et confirmare possumus, in animo est mihi proferre. Hoc modo igitur, ut spero, ex memoria hominum detrahere et oblivioni æternæ tradere istas opiniones falsas æque ac absurdas quæ tamdiu animis eorum arisere, nonnihil valebo.

Inter tot tamque diversas opiniones, quarum nonnullæ a viris illustribus traditæ sunt, quid sit verum, quid falsum, ut juvenem et tyronem decet, explorare conabor. Rem vero religioni non habeo, quod interdum a sententiis medicorum, etiam clarissimorum discederem. Me hisce verbis utente, quin invidiam, forsitan et contumeliam mihimet paravero, haud incertus sum. His autem magis ire obviam, quam falsum pro vero positum indecoro silentio præterire volo. Hujus rationem fluxus reddere vel potius attentionem nominum ad rationes exque-rendas incitare, mihi consilium est. Propter sententias ab aliis diversas, hac in dissertatione prolatas, me facere insignem, si possit fieri, non in animo est. Ne quis igitur vitio vertat, si opinionibus aliorum non semper accederem. Quâ modestia viriumque suarum diffidentiâ uti juvenem decet, suam de rebus omnibus non minus quam medicis, sententiam in lucem prodentem, non omnino ignoro. Hic vero recordari nos oportet, tyrones sæpe ac imperitos, quæ maxi-

mo sunt usui vitæque humanæ accommodatissima, multa feliciter invenisse. Hanc tamen rem adeo obscuram non confidentissime aggredior. Quod si quæ dicenda sint, ad justas causas mensium fluxus, vel, ut jam dixi, animos hominum eas explorare quoquomodo ducant, satis habeo.

Non ab re alienum videbitur, observationes tradere perpaucas de sentiis eorum qui de catameniis scripserunt. Imprimis autem, de signis et mutationibus quæ primum mensium fluxum præcedunt et comitantur, pauca præmittere necesse erit.

Vix dicere possum, an alia ulla in natura sit operatio, varios inter ordines sive plantarum sive animalium, quæ tam varios tamque diversos edit effectus, quam quas subit puella mutationes tempore pubertatis. Totum fere corpus mutatur. Forma puellæ universa magis fit gracilis, crescit magnitudine uterus, pubes venit, mammæ efflorescunt, pulchrior renidet vultus, splendent oculi, incessus, gestus, sermo, omnia denique fiunt magis decora.

Tempus, quo primum menses erumpunt, non omnibus idem est; variat enim regionis temperie, vitæ conditione forsitan et multis aliis causis. Maturius pubescunt puellæ in regionibus calidis, in frigidis vero tardius. In regionibus calidis, menses quoque multo citius quam in frigidis sese manifestant. Hoc puellis Asia-

ticis annum ætatis octavum vel nonum agentibus, accidere dicitur: quo tempore, fœminæ fiunt, priusque matres factae sunt, quam annum ætatis decimum compleverint. In regionibus frigidis multo serius accidit, neque, exceptis paucissimis exemplis, primum se ostendit ante annum ætatis vicessimum.

Quantum variat tempus quo primum eveniunt menses, in diversis regionibus, tantum variat quantitas sanguinis ejecti. In regionibus frigidissimis, tres uncias, in calidioribus usque ad sexdecim, per mensium fluxum, mulieres amittunt. Certam pro regionis situ proportionem ratam spectat hujus fluxus quantitas. In his regionibus, nempe in Britannia, sex vel octo unciae sanguinis, singulis mensibus, hoc modo, fluere censentur.

Puellæ Britannicæ, sana valitudine fruenter, circiter annum ætatis decimum quartum vel quintum, primum mensium fluxum patiuntur. Quo tempore, languor, et lassitudo insolita, pandiculatio, dolor lumborum, ventriculi, et capitis, vertigo, nausea, interdum et vomitio, tumor mammarum aliquando et dolor, aliaque his signa similia puellam fatigant. Hæc signa jam recensita mensium fluxum in genere præcedunt, duranteque nisi per intervalla breviora, donec sanguis ex cavo uteri prorumpit. Si vero, hoc tempore, aliquid exitui ejus per uterum obstet, fluxus est ex naribus, pulmonibus, ventriculo, ano, oculis, gingivis, mammis, umbilico, minimo manus

digito, aliquando et aliis insuetis corporis partibus.*

Sanguinem, ex utero, tempore mensium fluentum, esse sincerum, putaverunt nonnulli. De hac re, autem, maxime dubito: alii enim, hunc sanguinem menstruum non esse congelabilem, neque ullam habere similitudinem sanguinis sinceri naturæ, nisi colori ejus, asseruerunt. De his, autem, mox plura dicenda sunt.

Hic sanguinis fluxus, mulieribus per magnam vitæ partem naturalis est, et valetudini earum maxime necessarius, singulis mensibus occurrit, si uterum non gerunt, quo tempore, rarissime se manifestat. Non desunt qui, menses, more solito, per totum graviditatis tempus, permansisse memoriæ tradiderunt. Sed paucissimis in exemplis, quibus res ita sese habuerunt, tale profluvium pro hemorrhagiâ morbidâ censi debetur: forsitan et ex arteriis uterinis, minime effunditur, sed ex vasis in vaginam hi-antibus. Satis enim constat, quo tempore, conceperit mulier, os uteri, materiâ mucosâ, occlusum esse. Sanguis igitur, more mensium solito, ex cavo uteri gravidi, fluere non potest. Hujusmodi profluvia, maxima, tenero embryoni, incommoda, mortemque, fortasse ipsam, afferrent.

Menses quo ex tempore, mulieribus primum contingunt, certis intervallis redeunt, perque

* Vide De Graaf, page 179.

rium, quatuor, aliquando et sex dierum spatium fluentes, vix ante quadragesimum quintum et fere ante quinquagesimum ætatis annum desinunt. Sed quibusdam in casibus ad provectionem ætatem fluere perstant. Non desunt exempla, quamvis rarissima, quibus menses sub pubertatem proruperunt. Medicus ille Alexander Monro pater, de quâdam puellâ, mentionem fecit, cui menses occurrerunt, tertio quidem anno ætatis.*

DE CAUSIS EXCITANTIBUS.

De mensium causis, varias multumque inter se diversas opiniones, in scriptis medicorum invenimus. Multa quidem vana et futilia, tum de causis, tum de effectibus eorum, tradiderunt physiologi. Causæ eorum, ut quibusdam visum est, ex cursibus lunaribus deducendæ sunt, Alii, autem, probare conati sunt, eas in bile existere. Plerique, vero, inter recentiores, medici, de hâc rê disceptantes, vel ex congestionem sanguinis uterini, vel plethorâ corporis universalis, vel partibus corporis jam evolutis, vel arteriarum densitate, causas quæsierunt. Hodie quidem, multi, menses oriri, ex unâ vel alterâ harum causarum quas nunc enumeravi, se habent persuasos. Nostro autem judicio, qui putant, menses esse secretionem ex arteriis uterinis, quæ pendet ex conditione ovariorum peculiari, proprius ad veritatem accesserunt! Nunc vero, pauca, de rationibus, quibus, suas, de

* Vide, page 422.

hac re, opiniones confirmare potuerint medici, in lucem proferam.

Vix opus est verbis, opinionem refellere, de lunæ effectibus in mensibus ciendis. Si enim luna hoc efficere valeat, mulieribus omnibus, eodem fere tempore, menses contingerent. Sane res ita non fiunt. Namque, omnibus cursus lunaris temporibus, hunc mensium fluxum pati solent mulieres. Igitur, quam medicus Mead male finxit, de vi lunæ pro mensium causa opinionem ut falsam æque ac absurdam rejicere debemus. Quare autem rogare volo, menses non fluunt ante annum ætatis decimum quartum vel quintum, atque post quinquagesimum desinunt, quoniam corpus foemininum, per totum vitæ spatium, vi lunæ eadem semper afficiatur? Hic, ut mihi videtur, homines ad vana fingenda, quam ad rerum naturam investigandam proniores opinionibus tam absurdis fidem habere adducti sunt.

Alii vero physiologi, hac de re, disceptantes, hoc errore relicto, in alium abierunt. Scyllam cupientes evitare, in Charybdim inciderunt. Opinionem, menses originem ducere ex nimia bilis copia, vel ex natura bilis peculiari, non vero rationibus validioribus, neque observationibus magis certis, quidem protulerunt. Sed ut supra dixi, plura facere verba, has opiniones esse falsas ostendere, non erit necesse.

Ad aliam, de mensium causis, opinionem, animum advertamus. Menses ut quibusdam videtur, ex congestione sanguinis in uteri vasis, oriri censentur. Medici vero hanc opinionem proferentes, quo pacto fit, ut hæc sanguinis congestio, quæ, parvis incrementis in molem facta, certissimis intervallis prorumperet, non nobis dixerunt. Pro certo habeatur, ut plethora in aliqua corporis parte partialis brevi fiet universalis. Nunc autem de hac plenitudinē corporis universali, a plerisque hominibus pro mensium causis habita, fusius dicam.

Pauci quidem ante tempus praeclari Culleni, hanc opinionem ita investigaverunt, ut rei adeo obscuræ multum lucis attulerunt. Culle- nus vero ipse, quamvis, acumine ingenii solito, hanc rem explicare conatus est, tamen caligine atra obvolutam reliquit. Sententiam ejus pro mera hypothesis habere oportet; tamen lubentissime concedam esse hypothesis ingeniosissimam. Asserit ille utrum esse evolutum post alias corporis partes; et igitur hæ partes sanguini a corde demisso resistentes, in laxa vasa uteri eum nempe sanguinem determinant; hæc vasa cedentia impetui sanguinis impingentis, donec tandem rumpuntur, exitusque ad sanguinis fluxum datur; atque, ut ait ille, alia fit accumulatio per mensis spatium. Sic ille disseruit. Sed haud scio unde oritur hæc nimia sanguinis abundantia quæ mulieribus omnibus in eadem regione degentibus, eodem ætatis tem-

pore, congeritur, et singulis mensibus fluere perstat, donec arteriæ uterinæ tam firmæ densæ que factæ, sanguinis exitum omnino impediunt.

Ad opinionem eorum qui putant mensium causas oriri ex plenitudine corporis, quæ a quadam conditione ovariorum pendet haud facile accedam ; neque aliam plenitudinem præter naturam, in corpore existere, credere possum.

Ceterum affirmat Cullenus, arterias uterinas non esse equales, quoad firmitatem vel densitatem, arteriis aliarum corporis partium, sed laxiores et infirmiores esse : atque satis constat, incrementum omnis corporis partis ex toto pendere a fluxu sanguinis in eam demisso. Uterus vero, putat ille, tardius evolvitur aliis corporis partibus, et hoc tempore, determinatio est sanguinis ad uterum, cujus arteriis impetui sanguinis cedentibus, menses fluunt, et fluere perstabunt, solitis intervallis, donec arteriæ uteri tam densæ et firmæ fiunt, ut sanguini ab aliis corporis partibus demisso resistere valeat, quod accidit cum menses desinunt.

Nunc autem observandum est, quam maxime erravit Cullenus, cum rationes, de hac re redderet. Nam si aliæ corporis partes firmæ ac validæ factæ impetui sanguinis resistunt, et cum in vasa uteri laxiora et infirmiora determinant, non possit fieri, ut hæ partes, figuram justam, et firmitatem adeptæ, post mensuum

fluxum magis increscerent. Res vero aliter se habent.

Satis enim constat, totum fere corpus muliebri, postquam menses acciderint, magna incrementa accipere, et quibusdam in casibus, quoad formam, figuram, et firmitatem multum mutari. His igitur rite perpensis, fidem opinioni Culleni habere non possum, menses a successione evolutionis, ut dicit ille, pendere.

Ad quam protulit Charletonus, de hac re, opinionem, nunc animum advertamus. Putat ille, menses originem habere a quodam spiritu nitro aereo. Ejus verba sunt, "*Utut hæc res se habeat, mihi quidem verisimile videtur, spiritum nitro aereum in hoc opere choragum esse; habemus itaque tandem jam verisimilem saltem, si non verum menstrui ardoris in utero muliebri originem.*"

Valde miror, quomodo fieret, ut Charletonus, erroribus et falsis sententiis Hyppocratis, Galeni, aliorumque antiquorum, ab illo in lucem prolatis, repudiatis, opinioni tam absurdæ fidem haberet. Hic vero nobis attulit certissima documenta, quo facilius est hominibus, errores aliorum exponere quam veritatem investigare: male facere, qui vult, nusquam non causam inveniet.

Hisce igitur observationibus de sententiis aliorum jam traditis, rem, ut se habet, quoad po-

tero, in animo est mihi tractare. Atque ut solent sapientissimi mortalium, quum in rebus dubiis et obscuris investigandis versantur et viam ad veritatem tramite recto, persequi nequeunt, quam habent aliis rebus magis apertis et perspicuis analogiam invenire conantur. Horum exemplo adductus de re adhuc satis obscura, meam quoque sententiam in medium proferam.

Neminem latet, omnes humani corporis secretiones ex toto pendere a conditione organi discernentis perfecta, et prius organum ad conditionem perfectam pervenit, quam secretiones illi proprias perficere potest: cæterisque paribus, quo magis alicujus organi conditio, eo melius suas præstat functiones. Exempli gratia, conditio peculiaris hepatis, testium, et mammarum, pro bilis, seminis, et lactis, secretionem, maxime necessaria est. Haud scio, quibus ex causis oriuntur menses, si non signa censentur certa, quibus certiores facti sumus, utrum esse perfectum et ovaria evoluta, atque ad munera sibi propria perficienda, apta. Menses semper fluxerunt, ante quam mulier uterum gerit. Nostro autem judicio, non pro causis, ut nos docuit professor Simpson, sed tantum pro signis plenæ pubertatis hic fluxus haberetur.

Ratio igitur in promptu erit, cur menses mulieribus non citius contingunt. Quare, rogare volo, neque semen secernitur pueris, neque lac puellis, quinto vel sexto ætatis anno? Huic res-

pondeatur, quia testes et mammæ, organa istos humores secernentia, ad conditionem perfectam, qua suis propriis muneribus fungantur, non adhuc pervenerunt.

Sed, ut supra memoravi, nonnulli auctores, tum veteres, tum recentiores se habent persuasos, menses oriri ex plenitudine corporis vel universali vel partiali, quæ, tempore pubertatis maxime valet. Quo fit, ut omnibus puellis certa ætate, inque eadem regione, menses acciderint, difficile erit ex hac opinione rationem reddere. Satis enim constat, ut puellæ, quamcunque vitam agant, vel qualicunque victu utantur, hunc mensium fluxum patiuntur. Et si quando menses, solito tempore, non fluxerint, tamen naturæ conamina ad hoc efficiendum non omnino deficiunt.

Unde igitur oritur hæc corporis plenitudo quæ longe lateque grassatur; cui obnoxia et opportuna sunt omnes puellæ, cujuscunque temperamenti, sive vitam in urbe aut in rure agerent, sive laute viverent, aut tenui et parciore victu uterentur?

Si autem menses neque ex lunæ imperio, neque bile, neque spiritu nitro aereo, neque ex plenitudine corporis proficiscuntur, quibus ex causis oriuntur? Qua ex causa oritur seminis secretio ex vasis spermaticis? Certe ex conditione testium fæcundante. Eodem quoque mo-

do, mensium fluxus a conditione ovariorum fecundante pendet. Fluxus iste igitur censeri debet secretio ex arteriis uteri quibus stimulo est conditio ovariorum perfecta. Joannes Bell eandem opinionem prælectiones suas anatomicas audientibus tradidit.

Professor Munro, medicus celeberrimus, cum sententias de hac re, in suis prælectionibus utilissimis proferret, menses a peculiari ovariorum conditione pendere, asseruit. Idem vir illustris addere vult, hanc conditionem ovariorum plenitudinem corporis universalem efficere.

Certe scio, cum discederem a sententiis hujus viri clarissimi, ut homines arrogantiam, fortasse et ignorantiam, mihi attribuent. Mihi vero non est consilium, si fieri possit, aliquid detrahere nomini ejus, quem plurimi facio, et maximum et solertissimum medicum lubentissime agnoscō. Non cupidus novi, sed rerum causas cognoscendi, ab illo dissentio. Et si in hoc erraverim, omnes bonos ac candidos iterum iterumque oratos volo, ut ne mihi vitio vertant, et me amore veritatis captum in errorem adductum esse meminissent.

Cum Professore Munrone, de hac re, doctissimo et ingeniosissimo Professore Jacobo Gregory in sententiis prælectionum suarum traditis, convenire placet. Putat ille quoque plenitudinem corporis, ex qua oritur mensium fluxus,

quadam conditione ovariorum effici. Sed quomodo fit hæc plenitudo in corpore, neque hic, neque ille, demonstravit, et quomodo fiat confiteor me omnino ignorare.

Diffidens animi hanc rem aggredior; namque bene scio meas vires eam tractare, ut vellem, non sufficere. Tamen nos oportet cavere ne addiceremur in verba jurare ullius magistri; me enim judicante, multum est periculi, et haud ignarus sum, ut medici et philosophi nomina et auctoritates magnorum virorum pro validis rationibus sæpe adducunt, et nos opinionibus eorum magnis nominibus et auctoritatibus tantum firmatis assentire volunt. Non quidem est tyronis, sine ulla investigatione fidem habere sententiis aliorum. Si enim res ita esset, vera scientia semper fuisset inculta; de circulatione sanguinis, de curatione variolæ, aut de vasis lymphaticis, nostras ad aures nihil unquam pervenisset.

Nunc igitur in animo est mihi meas sententias, de mensium causis, proferre, ut alios majore ingenii acumine dotibusque animi præstantioribus præditos operam dare huic rei investigandæ, excitem. Quamvis enim nonnulli putaverunt rem tam obscuram nunquam in lucem proferri posse; tamen sperare possum, tempus non multum abesse quum erit manifesta; et fortasse invenietur, menses oriri ex conditione ova-

riorum fecundante, quæ est stimulo arteriis uteri, ex quibus iste humor secernitur.

Multa quidem animum ad hanc opinionem ducunt. Nullas novimus puellas viro tempestivas, quibus menses non accederint. Aliis quoque animalibus lex est hujusmodi.*

In priore parte hujus opusculi, jam observavi menstruum sanguinem non esse coagulabilem more sanguinis sinceri, et huic sanguini nullam habere similitudinem nisi rubedini ejus. De hac re, autem, nunquam mihi dabatur occasio ulla experimenta faciendi. Ex experimentis aliis institutis hoc accepi. Quamvis autem sanguis menstruus esset sincerus, ex hoc demonstrari non potest, plenitudinem adesse, vel quomodo arteriæ uterinae partem sanguinis a generali massa separare possunt.

Si vox plethora audit justam et ratam sanguinis quantitatem, quæ distentioni et actioni vasorum necessaria est, non ut vulgo significat conditionem corporis morbidam, in hoc sensu vox est absurda. Plethora in posteriore sensu tantum notat conditionem morbidam et laxitatem solidorum corporis indicat. Si accuratius dicere volumus *vi* aut *stimulo* uti debemus, quo facilius lis dirimi possit.

Satis constat, actiones omnium arteriarum a sanguine in iis contento ex toto pendere. Iis

• Vide Spalanzani.

sanguis est stimulo, quo functiones earum rite praestent. Haec quantitas sanguinis vasa stimulat in eodem modo ut calor, et haec quantitas sola necessaria est ovaria stimulare ad munera propria perficienda. Ovaria sunt stimulo arteriis uterinis quae humorem menstruum se cernunt.

Quo tempore primum menses erumpunt puellis, pubes venit, mammae turgent, incalescit uterus, genitalia suis functionibus apta sunt, quæ omnia nobis sunt certissimis signis jam esse viro maturas. Si quando, ut supradixi, fluxus sanguinis per uterum impeditur, fluxus est ex ventriculo, pulmonibus, oculis, aliisque corporis partibus. De his autem mox fusius dicam. Nunc pauca de recursibus mensium periodicis proferam.

Plura dicere, de opinione eorum qui mensium fluxum a lunae imperio pendere putaverunt, supervacaneum est. Quod dicendum est, de plenitudine partiali, æque ad universalem spectat. Omnes enim affectiones hujusmodi primo partiales, tandem fiunt universales. De hisce igitur opinionibus, hoc in loco, simul agere in animo est; utpote sunt opiniones omnium fere medicorum, nonnulli quorum ante tempus Culleni plenitudinem esse corporis universalem demonstrare conati sunt.

Cullenus de plethora hypothesin finxit ingeniosam. Asseruit ille, primum mensium flux-

um a densitate et firmitate arteriarum aliarum corporis partium, quæ sanguinem ad uterum determinant. Arteriæ uteri jam evoluti laxiores et infirmiores impetui sanguinis cedentes, tandem rumpuntur et sanguis effunditur. Per intervallum mensis, alia est congestio, aliusque fluxus; atque ut solet accidere aliis hemorrhagiis, recursus hujus fluxus, certis intervallis consuetudine confirmatus est.

Cullenus autem, quo fit, ut hic fluxus tam accurate recurrat mensis intervallo, aut omnibus fere foeminis in eadem regione, eadem essent intervalla, non demonstravit: nec quidem demonstravit, quomodo fit recursus alicujus hemorrhagiæ certis intervallis.

Verbi gratia, fingamus plenitudinem adesse, atque octo uncias sanguinis per mensis spatium accumulari, et ex utero fluere. Cum ratione conveniet, si sexdecim unciae sanguinis, die decimo quarto ante tempus solitum, per venae sectionem auferantur, non erit fluxus antequam duo menses elapsi fuerint, vel mensis cum semisse. Si igitur causa hujus fluxus occasionalis ex congestione per mensis spatium facta originem habet; duplici sanguinis quantitate per mensis intervallum detracta, triplex fere temporis spatium elabi ante alium fluxum, necesse erit: quippe quod tempus duorum mensium ad congerendam sanguinis quantitatem detractatione missam requiritur et prius pars tertii mensis

elapsa fuerit, quam congestio et fluxus iterum acciderent. Sanguis autem, hoc modo, missus, neque tempus neque quantitatem mutare potest. Medicus Simpson, ni fallor memoriae tradidit, decem libras per venæ sectionem missas, quantitate et intervallis menstrui fluxus iisdem manentibus.

Ex his igitur, ut mihi videtur, menses a plenitudine corporis non pendere, manifestum est. Aliud autem est argumentum ex eo quod accidit foeminae cui per errorem ovaria excidit Percival Pott. Ante hoc tempus huic foeminae menses rite et copiose fluxerunt. Postquam autem res tam infelix evenerat, mammae subsiderunt, omniaque signa solita mensium fluxum comitantia desiverunt, seque nunquam postea ostenderunt. Hisce igitur animo volventibus, dubitare non possum, quin menses majis pendeant a conditione ovariorum, quæ est stimulo utero, quam a plenitudine corporis.

Nunc quomodo redeunt menses certissimis intervallis explicare oportet: Confiteor tamen, ut rationem hujus rei obscuræ satis validam reddere non possum; quamvis me habeo persuasum ut explicari possit et rationem non multum abesse. Cur non redeunt gratæ vices diei noctisque intervallo hebdomadis vel mensis et non diei? quia legibus hominibus aliisque animalibus impositis, stimulus ad suas functiones perficiendas necessarius, paucis horis diminui-

tur, et quies et sopor ad eum reficiendum singulis diebus, requiruntur. Eadem ratione, hic fluxus singulis mensibus contingit foeminis. Si enim semel per anni spatium tantum accidisset, guadia hominis multum imminuta essent, et si nunquam accidisset, foeminae nunquam uterum gessissent: quia mensium fluxus ad secernendos humores faecundos necessarius esset. In aliis animalibus humor istiusmodi qui ex utero fluit non omnino deest.

Quo fit ut arteriae uterinae per paucas dies singulorum mensium hunc humorem peculiarem secernunt? Eodem modo quo secernitur lac ex arteriis mammarum post omnem partum. Putant nonnulli rem esse supra fidem tantum hujus humoris ex arteriis uteri secerni, quantum paucis diebus ex utero fluit. Non mirum est, si credere possumus quantum seminis secernitur ex spermaticis canis pro rato tempore in coitu.

Menses, ut observatum fuit a nonnullis auctoribus ex aliis corporis partibus fluxerunt; eodemque modo, quibusdam in casibus, bilis per vesicam transit. Hic, hepatis, ille, uteri, morbidam conditionem demonstrat. Interdum quoque bilis per ductum cholidochum impediatur.

Nunc pauca dicenda sunt, cur menses circiter annum aetatis quadragessimum quintum vel quinquagesimum annino desinunt. Qui opinati sunt menses ex plenitudine oriri, quædam

argumenta hoc ex fonte deduxerunt. Illi putant, menses cessare hoc tempore, ex firmitate arteriarum uteri quæ sanguinis impetui resistunt. Vix credere possumus, arterias omnium foeminarum eodem tempore ætatis, æque esse firmas ac densas, aut fieri minus capaces quo sanguinis fluxum impediunt. Haud quidem veri simile est. Alia ex causa oritur, ut mihi videtur, nempe, quia, hoc tempore, ovaria conditionem faecundantem omnino omittunt. Namque providens natura posuit legem, ut menses fluere cessarent, et ovaria conditionem quæ est stimulo utero amitterent, cum mulieres non diutius nutrimentum faetui præbere possunt.

DE CAUSA FINALI.

Adhuc quidem in dubio est, quibus usibus inserviant menses. Multi putant esse nutrimento faetui. Nonnulli, tum veteres, tum recentiores, hanc opinionem protulerunt. Quia menses desinunt, dum uterus est gravidus, putatur, sanguinem qui erumpere solebat, per mensium fluxum pro nutritione embryonis, deteneri.

Per aliquod temporis, postquam mulier conceperit faetus in utero fabæ aut vespæ vix æqualis est magnitudine. Hoc tempore igitur minima nutrimenti quantitas satis esset. Quo vadet nunc ista pars sanguinis quæ nutrimento faetus superest? Si sanguis menstrualis huic usui dicatus fuerit, principio graviditatis

nimis abunderet, ad finem vero multum deficeret. Sunt autem multa alia animalia quae menses non habent; tamen faetus eorum non orban-
tur nutrimento. Cur igitur auctores crediderunt, menses esse nutrimento faetui in humano genere, cum aliter res se habet in aliis animalibus? Ratio in promptu est, quia veras mensium causas non investigarunt quidam auctores putaverunt menstruum sanguinem esse foedum et perniciosum. In legibus Leviticis, de hac re, nonnulla sunt praecepta. Hac vero, ut mihi videtur, ad munditias populi istius, quam ad aliquid immundum vel lethale in menstruo sanguine magis spectant.

Igitur, ut res mihi apparet, consilium est naturae in mensibus ciendis, ut certa sint signa uterum esse maturum. Ex hoc explicare possumus, cur menses desinunt, dum uterum gerit mulier; quia hoc tempore, ovaria amittant conditionem faecundantem, quae est stimulo arteriis uteri. Itaque menses tempore graviditatis omnino desinunt, nisi in paucissimis casibus hisque morbidis.

Quam finem dicendi fecerim, ante meo amico et socio Joanni Thomae Shaaf medicinae Doctori, aliisque omnibus amicis maximas agam gratias, ob multa et magna beneficia in me collata.

- “ Ante leves ergo pascentur in aethere cervi
 “ Et freta destituent nudos in littore pisces ;
 “ Ante, pererratis amborum finibus, exul
 “ Aut Ararim Parthus bibet, aut Germania Ti-
 grim
 “ Quam nostro ipsorum labatur pectore amor.”

TANTUM.

OF THE
CAUSES
OF THE
MENSTRUAL ACTION.

Which of the various subjects, appertaining to Physick, and suitable to my present purpose I should chuse, and that on which I could enter with the greatest prospect of success, I for a considerable time and seriously doubted. But, maturely and calmly weighing all things, I have resolved to offer a few remarks on the Menstruous Fluid of the human female. From what causes this sanguineous effusion has its origin; or what is the particular end of the economy of nature in establishing the menstrual operation in the female body, neither Physician nor Physiologist, has as yet ventured confidently to assert. Among the various faculties and functions of animal life there is none more difficult of explication. Many Physiologists, it is true, as well ancient as modern, have turned their attention to this subject, but in my estimation, if we

except a few of the present day, they have stopped very far short of success ; their labour has been lost.

To adduce any thing altogether new on this subject is not my intention ; my object reaches no farther than to advance a few arguments, which may tend to give stability and firmness to the sentiments referred to by Hunter, by Denman, and by one or two other medical men. In this manner, I flatter myself, I shall be able, in some measure to loosen the attachment of gentlemen from those false and absurd notions, which have so long amused their fancies, and diverted their judgments, and to turn over to eternal oblivion those speculations themselves.

As becomes a young man and a learner, I shall endeavour to discriminate between the true and the false of the numerous and various opinions which have come down to us ; some from illustrious characters. But I shall not hold it to be a point of conscience never to depart from the path of other Physicans, even the most celebrated. In adopting this language, I know that I shall procure to myself censure, perhaps contumely : but I had rather run the hazard of all such consequences than silently to permit error to occupy the place of truth. My object is an enquiry into the nature of the menstrual operation ; or rather to awaken the attention of others to the research. But I have no expec-

tations of acquiring fame by a mere opposition of sentiment. Therefore let gentlemen be sparing of censure although I should not accord exactly in opinion with those who have preceded me. I am not now wholly to learn that it is becoming a young man not only in medical, but all other matters, to come forward with a proper degree of modesty, and distrust of his own powers ; nevertheless we must never lose from our recollection, the important fact, that some of the most valuable and useful discoveries have been fallen on by young, and even ignorant men. However I shall approach a subject so obscure with no slender caution, and if what I shall say may lead to a discovery of the true cause of the Menstruous fluid, I shall feel satisfied.

To deliver a few remarks on the sentiments of those who have turned their attention to the menstrual operation, may not be deemed irrelative to our subject : and especially it may be necessary to premise a few observations on the signs and phenomena which precede and accompany this eruption.

It is highly problematical, whether there be in the vegetable or animal kingdom, any natural operation in which there is so remarkable a revolution as that to which a young maid is subject at the period of pubescence. Almost the whole body is changed. The universal form of

the virgin becomes more soft and winning, the uterus enlarges, the marks of puberty appear, the bosom evolves, the countenance is more beautiful, the eyes sparkle, the step, the carriage, the speech; in short all is grace and elegance.

The time at which the menstruous action takes place is not the same in all. It varies according to climate, conditions of life, and various other causes. Girls in the warmer regions are pubescent sooner, those in the colder later. The same is also true of the eruption of the menstrual fluid. This change is said to occur with the girls of Asia so early as during the eighth and ninth year; at which time they are women, and become mothers before they shall have completed their tenth year. But in the colder latitudes, this change does not occur until much later; nor indeed, except in very few instances, previous to the twentieth year.

The periods at which the eruption takes place in different latitudes, do not differ more than the quantities discharged. In the very cold, about three ounces, in the very warm, to the extent of sixteen, are thrown off. The quantity bears relation to the latitude. In this climate, viz. that of Britain, from six to eight ounces of blood are supposed to be the mean quantity discharged.

The girls of Britain, enjoying good health, for the most part ; experience this function about the fourteenth or fifteenth year of their age. At this epoch they are more or less annoyed by unusual languidness, and weariness, a disposition to yawn, pains of the loins, of the stomach, and of the head, giddiness, inclination to vomit, and sometimes absolute vomiting ; tumefaction, and, at times, pain of the breasts, and other similarly uncomfortable feelings. Those sensations, in a greater or less degree, continue at intervals, until the fluid begins to pass off from the womb. But should any impediment oppose its passage through the womb, at this time, it has been observed to flow from the nose, the lungs, the stomach, the fundament, the eyes, the gums, the nipples, the navel, the little finger, and other unusual parts.

The red fluid which flows from the womb during these particular periods has been supposed by some to be pure blood. But of the correctness of this supposition we very much doubt. For it has been advanced and maintained that it is not coagulable, and that, except in colour, it has none of the sensible properties of pure blood. But of this more presently.

This discharge of red fluid is proper to women during a considerable part of life, and is very necessary to the maintenance of health. It occurs monthly, the periods of gestation be-

ing excepted, at which periods it very rarely takes place, but instances have been furnished where this monthly discharge has regularly, in its usual form, occurred during the whole term of gestation. The instances, however, are very few in number where matters have been so uniform and regular; and, when they do arise, should be viewed as morbid: and perhaps in those rare cases the fluid may more properly be imagined to be furnished by the vessels of the Os Tincæ opening into the Vagina than by the uterine vessels belonging to the hollow of the womb. For it is beyond a doubt that, at the time of conception the mouth of the womb is closed by mucous matter. Therefore the fluid, in the customary way in which it passes off, cannot be discharged from the inside of the womb. An effusion in the usual way would necessarily bring a serious incommodity on the child, if indeed it did not endanger its life.

This menstruous fluid, from the period at which it first flows, returns periodically every month, and continues at each return, about from four to six day, does not wholly cease until the forty-fifth or fiftieth year of the woman's age. There are not wanting examples, although extremely rare, where this menstruous operation occurs under puberty. Dr. Murno the father, makes mention of a little girl, in whom this operation took place even at the tender age of three

years, and continued regularly thereafter through the ordinary space of time.

OF THE EXCITING CAUSES.

We meet with various and greatly different opinions in the writings of Physicians on the causes of the menstrual operation. Indeed Physiologists have amused themselves with many conceits and visionary speculations as well on the causes as on the effects of this operation. Some have traced the causes up to an imaginary influence of the moon, and others have laboured to prove that they resided in the qualities of the bile. But most of the moderns who have treated of this matter have attributed the causes either to a local congestion of blood in the uterine vessels or to a general plethorick condition of the whole body, or to an unequal evolution of the different parts of the body, and density of the arteries. At present indeed there are but few who do not consider the menstrual action to depend on the one or the other of the causes recited. But in our estimation those who view this fluid as a *secretion* from the uterine vessels under peculiar ovarious influence, approach much nearer the truth. And we will now detail a few of the reasons, by which, the advocates of this opinion might establish their sentiments.

A few words may suffice to set aside the conjecture of the menstrual operation being attributable to lunar influence. If the moon ruled, in this affair, it is presumable that all women would be under the operation at the same moment. This is not the fact. For at every period of a lunation some one or other is under this operation. Therefore this visionary notion of Dr. Meed is to be rejected both as unscientifick and futile. We would ask, on this head, why does not the menstrual action take place before fourteen or fifteen or why does it cease after fifty years, seeing that the female is during the whole of life equally exposed to lunar power? Here, as it appears to me, we have a striking illustration of the greater disposition to indulge in absurd and ridiculous conceits than to engage seriously in the sober investigation of the real nature of things.

But other Physiologists, keeping void of one error, have fallen on its opposite; desirous of avoiding Scylla, they have been wrecked on Charybdis. The opinion advanced by certain gentlemen that the menstruous operation derives its origin from too great a quantity or a peculiar quality of the bile rest upon ground as untenable and precarious as the sentiment noticed above. And to say more to demonstrate the incorrectness of those opinions would certainly be a waste of words.

We will direct our attention to another hypothesis touching the subject. The menstrual operations have been conceived to depend on a congestion of blood in the uterine arteries. But the Physicians who have favoured this conjecture have not instructed us how it comes to pass that congestion accumulating by slow degrees into a considerable mass should burst forth at given intervals. And it may be received as an incontrovertible fact that every partial plethora, in any part of the body must in a short time become general. Therefore we shall speak somewhat at large on the general fulness held by almost every physiologist as the unquestionable cause of the menstrual operation. Few indeed antecedent to the time of the eminent Cullen had so investigated this opinion of plethora as to throw any considerable light on the subject. And Cullen, although not deserted by his usual acumen, unsuccessfully essayed this difficult point having found it immersed in great obscurity. Indeed we are permitted to grant this opinion of Cullen no higher rank than a hypothesis yet we most willingly acknowledge it to be an exceedingly ingenious one. He lays it down as his premise that the uterus is the last part of the body which is evolved, and consequently that the blood sent out from the heart, being resisted by the superiour strength of the arteries of the other parts, is determined into the laxer vessels of the womb, and these gradually

yielding to the force of the impinging mass ultimately give way, and an effusion of blood is the result. This course, of things in the course of a month is repeated. Thus he taught. But I am as yet to learn whence this too great abundance of blood, in all women in the same climate, and at the same epoch of life is collected and continues to pass off, at every four weeks until the arteries shall become so firm and dense as altogether to resist its egress.

To the sentiments of those who suppose this plenitude of the general body to arise out of a peculiar condition of the ovaries I cannot readily accede; nor can I grant that the body at these periods possesses any preternatural repletion. But Cullen affirms, that the uterine arteries, as to firmness and density, are unequal to the arteries, of the other parts of the body; that they are more lax and feeble: and it is very obvious that the augmentation of bulk in every part of the body is entirely owing to the quantity of blood conveyed to it. The womb, as he supposes is of slower growth than the other parts, and, at the period of inequality, there is a determination of blood to this viscus, the arteries of which giving way there is an efflux of blood, which efflux continues to recur at certain intervals so long as the weaker or laxer condition of the arteries shall permit or so long as they shall

be overcome by the force of the blood sent from other parts ; when the balance is established the menstrual action ceases.

We must now notice the egregious error of Dr. Cullen in his philosophy on this point. If the other parts of the body, having acquired their tone and firmness, resist the force of the impinging blood, and determine it into the more lax and weak vessels of the womb, it could not happen that those parts already possessed of their proper figures and tone could acquire additional volume and power after the effusion of this fluid. But the fact is otherwise. For it is notorious that, almost uniformly, the whole body, subsequent to the eruption of the menstrual fluid, does receive great increase of bulk, and, in most cases, as to form, figure, and tone, does undergo great change. Therefore we cannot, when maturely considering these facts, assent to the opinion of Cullen, that this fluid is ascribable, in its cause, to any succession of evolution of the several parts of the body.

We now turn our attention to the opinion of Charlton on this subject. He supposed that the menstruous fluid, in its causes, was attributable to a certain nitro-aerial spirit. His words are : " howsoever the thing may be, I will not determine, but it appears propable to me that a nitro-aerial spirit is the chief furnisher or setter forth in the work ; therefore we shall consider it

as highly probable if not an absolute certainty that we have arrived at the true source of the meustrual ardour."

We are not a little surprised to find Charlton, after rejecting the errors and incorrect notions of Hippocrates, of Galen and of others among the ancients, by him so carefully introduced, admitting an opinion so positively absurd. But we are hereby furnished with the most unquestionable documents of the fact with how much greater facility we can successfully engage in the overthrow of the errors of others than in the establishment of truth on our own part. It is an easy matter to pick a hole in any man's coat.

Having advanced so far in discussing the sentiments of others, I shall now proceed, as my powers may enable me, to give my own views of this subject

And as the most learned in investigating that which is doubtful and obscure, when they could not proceed by a direct and known path, have endeavoured by the analogous and that which approaches most to the open and plain. So I, conducted by their example, shall state my opinion on this very obscure and difficult subject.

It is palpable and obvious that all the secretions of the human body depend entirely upon the matured condition of the secreting organ.

and that every organ before it can perform the function proper to it, must arrive at a state of maturity; and, other things being equal, that in proportion to the maturity of an organ will be the perfection of its function. For instance, a mature condition of the liver, of the testicles, and of the breasts, is altogether necessary for the secretion of the bile, of the seminal fluid, and of the milk. Nor can I conjecture any other source of the menstruous fluid, except it is to be viewed as signs by which we are to understand that the womb is so prepared and the ovaries are so evolved as to be suited to the affairs for which they are destined. The menstrual action always takes place before a woman can conceive. But, in my opinion, is not so much to be considered as the cause, according to professor Simpson, as sign or result of full puberty.

We therefore feel at no loss about the reason why the menstruous fluid does not occur sooner in women. Why, we will ask, is not the seminal fluid secreted by boys, or milk by girls, at the age of five or six years? To this it may be answered that, at this age, neither the testicles nor breasts, the organs concerned in these functions have arrived at that degree of perfection necessary to the performance of these offices.

But as above remarked, there are certain writers, as well ancient as modern, who have been

induced to believe that the menstrual operation is attributable to a partial or general plenitude of the body which in an especial manner prevails at puberty. Although the premises be granted, it will be difficult to understand how it comes to pass, that all girls at a fixed period of life and in the same climate, should have this monthly discharge. For the truth is that all girls without exception, what life soever they lead or food they use are subject to this operation. And if it should so happen that the menstrual fluid does not pass off at the usual period the efforts of nature to accomplish such an end will nevertheless come into operation.

Whence arises this fulness of body which is so general and uniform, and to which all girls are so obnoxious, of whatever temperament they may be, whether they pass their lives in the city or in the country ; whether they live luxuriously or whether they live on a spare and slender diet ?

But if the menstruous flux be not attributable to the influence of the moon, or to the bile, or nitro-aerial spirit, or to a fulness of body, from what causes does it arise ? From what causes proceeds the secretion of semen in the spermatick vessels ? Assuredly from the impregnating capacity of the testicles. In the same manner, also does the menstrual action depend on a condition of the ovaries to be impregnated.

Hence this flux is to be received as a *secretion* from the uterine arteries acting in obedience to an excitement derived from a matured condition of the ovaries. In his prelections Jno. Bell, delivered sentiments of this cast to his pupils.

Professor Munro, a physician of the greatest celebrity, when discoursing to his pupils on this subject, asserted that the menstruous action depended upon a peculiar condition of the ovaries. This same illustrious man added that this condition of the ovaries produced in the female body a general plethora.

I am perfectly aware that, when I dissent from the opinions of a character so celebrated, I may be charged with arrogance, and perhaps ignorance. But it is not my intention if the thing were possible, to detract from the name of him, whom I most willingly acknowledge to be of the first standing, and, of physicians the most learned and sagacious. Nor is it from a fondness of novelty that I differ from this gentleman, but solely for the sake of enquiring into the real cause of the thing. And should I err in this, I entreat all good and honest men, again and again, that they do not censure me but bear in mind, that captivated by the charms of truth, I have gone astray.

With Professor Munro, the very ingenious Professor Jas. Gregory agrees, on this head, as may be seen in his lectures. He also imagined

that the fulness of body, on which the menstrual action depended, arose from a peculiar condition of the ovaries. But how this plenitude of the body was produced, neither Munro nor Gregory has demonstrated, and how it can take place I confess myself totally at a loss to conjecture.

On giving my own opinion I now enter with diffidence, for I well know that my powers are far inferior to my undertaking. Yet it becomes us to be careful to avoid too strict a regard for the authority of our teachers, for in my opinion, and of the position I am thoroughly satisfied, both Physicians and Philosophers are often disposed to substitute names and authorities for solid reasoning, and are solicitous that we should admit their opinions because they are coupled with great names and high authorities. But it is not the province of a learner, without investigation, to pay too much respect to any opinion. For if so, true science would ever have remained rude and uncultivated: of the circulation of the blood; of the cure of the small pox; of the lymphatick vessels, no knowledge would have been handed down to us.

I now therefore shall bring forward my sentiments that I may at least rouse, those of better genius and more enlarged intellect to an investigation of the matter. For although some despair and believe a scrutiny into so obscure a subject must ever remain fruitless. I cannot be persuaded that the period is distant when this mat-

ter will be developed; and perhaps it will be discovered that the menstrual fluid does depend on a disposition of the ovaries to be impregnated, and is a *secretion* from the uterine vessels acting in obedience to this ovarious influence.

Many reasons indeed urge us to the adoption of this opinion. We know that girls are not marriageable until this operation shall have taken place. Even some inferior animals are subject to a similar law.

In the former part of this treatise, I remarked, that the menstuous blood was not, like genuine blood, coagulable, and that it possessed no character in common with pure blood but its red colour. On this, however, I have never had occasion to experiment. I rest upon the experiments of others. But even admitting it to be genuine blood it would not follow that a fulness must be present; nor is it clear how the arteries of the womb could regularly separate a given quantity from the general mass.

If the word *plethora* means a just and proper quantity of blood, such as is necessary to the distention and action of the vessels, and not, as is commonly understood, a morbid condition of the body, it is absurd. Plethora, in the latter and proper sense, indicates only a morbid and relaxed condition of the solids of the body. If we would speak more circumspectly, and, when treating of the quantity of the blood, use the

word *power* or *excitement*, we might more readily come to a mutual understanding.

It is sufficiently obvious, that all the arteries must depend entirely on the blood contained in them. This blood serves as a source of excitement to them, enabling them to perform their proper offices.* A given quantity of blood stimulates vessels in the same manner the heat does. And this given quantity alone is necessary to stimulate the ovaries to the performance of their function. The ovaries are a source of excitement to the uterine arteries which *secrete* the menstruous fluid.

At the period at which the menstrual action first accours young girls are pubescent; their breast enlarge, the womb is excited, their genitals are evolved, and all the attendant phenomena give assurance of nubile maturity. If at any time, as above hinted any impediment be opposed to its egress from the womb, it flows from the stomach, lungs, eyes, or other parts of the body. But of this matter I shall speak more at large presently. I will now say a word or two on the periodical recurrences of this fluid.

To dilate on the opinion of those who imagine the menstrual action to depend on the influ-

*The author wishes it also to be understood that the blood is the material out of which the secretions, subservient to the animal economy, are made.

ence of the moon would be superfluous; and what could be said of partial might also be urged in behalf of general plethora; for all partial affections of this sort, must of necessity become general. It is therefore my intention to treat of both these opinions at the same time; especially as almost all the attempts of Physicians, some before the days of Cullen, have tended to prove the position of the general fulness of the body being the cause.

The opinion of Cullen is ingenious. He affirms the effusion to be referrible to a given density and tone of the arteries of the general body, which determine the blood in upon the womb, the arteries of the evolved uterus, laxer and weaker than those of the body in general, yield and, ultimately giving way, a hemorrhage is the result. In the space of a month another congestion is established, and a second effusion takes place, and so on as with other hemorrhages: thus the menstruous flux, in its recurrences, becomes established by habit! Yet Cullen has not ascertained to us how it is that this flux recurs so precisely at the end of every month in every healthy female body, in the same climate, nor indeed is it proven by him that any hemorrhage does return at fixed periods with undeviating precision.

We will admit, for the illustration of the thing that the quantity of eight ounces is accumulated

in the course of four weeks, and passes off. It is presumable that, if sixteen ounces were taken away by art, there would not be a discharge until at least two or two and a half months should have elapsed. If therefore this occasional effusion be deducible from a monthly accumulation, a double quantity being artificially removed in the space of a month nearly triple the time must go its round before a second effusion, seeing that the space of two months will be required to recover the quantity lost by art, and a part of the third before the congestion could be complete and the effusion take place. But blood abstracted in this way and to this amount will neither change the period nor alter the quantity. Professor Simpson, if my recollection be correct, asserts that he knew ten pounds to be removed by the lancet, and yet the menstruous fluid remained the same in quantity and flowed at the customary intervals.*

From these facts, it is very clear, in my estimation, that the menstrual action cannot depend on plethora. And in addition to what has been said, the circumstance of the young woman from whom Percival Pott by mistake removed the ovaries might be adduced. Antecedent to the operation this young woman had had her men-

* The author, from much and extensive observation since the above was written, is decidedly of the opinion that the ground occupied is fully tenable, and to the doctrine no respectable fact can be opposed.

strual operations regularly and at each time copiously, but after this unhappy affair, her breasts wasted away, and all the phenomena attendant on those periods disappeared and never afterward returned. Attentively weighing these matters I cannot hesitate to make up my mind in favour of the opinion that this effusion is attributable to a peculiar condition of the ovaries serving as a source of excitement to the vessels of the womb, rather than to the doctrine of repletion of the body.

It now becomes my business to explain how it is that the menstrual action recurs at fixed periods. Yet I confess there will be a difficulty in giving an explanation clear of exceptions. Although I am fully persuaded that an elucidation can be furnished, and that I am not much out of the way in my present view. Why do not agreeable vicissitudes of day and night return weekly or monthly instead of once in twenty-four hours? Because by *established* and *immutable* laws imposed on man and the inferior animals, the excitement necessary to the performance of the functions of life is more or less diminished in a short space of time, and rest and sleep are required every twenty-four hours for its restoration. The same reason holds with regard to this operation in women. For if it did not recur more than once annually, human pleasure would be abridged, and if it never occurred, woman, according to her present constitution, would be barren, seeing that this operation

is absolutely necessary to the aptitude for fecundation. In some of the inferior animals there is something analogous.

How is it that the uterine arteries take upon themselves to secrete this peculiar fluid for a few days each month.* Upon the same condition that those of the female breast secrete milk after each parturition. Some have brought themselves to believe that it is improbable the arteries of the uterine could secrete, in a few days, so much as is perceived to pass off. It will be no longer wonderful after we satisfy ourselves of the immense quantity the dog will furnish in a measured space of time during coition.

It has been remarked by some writers that the menstruous fluid is not unfrequently discharged from other parts of the body than the uterus.† It is also a fact that the bile, at times, passes off by the urinary bladder. This points out a state rather morbid in the liver, that an unhealthful condition of the womb. The bile is also sometimes arrested in its passage through the duct common to the liver and the gall-bladder

* By fixed, fundamental laws of the female economy.

† This is congruous with the ordinary fact, that any set of arteries in the body may perform a vicarious office. They all in turns, assume to themselves the function of ossification.

We now turn our attention to the circumstance of the total cessation of the menstrual operation about the forty-fifth or fiftieth year. Those who have favoured the opinion of this action depending on plethora have not been inattentive to this fact. They supposed that this discharge ceased at this epoch, because the arteries of the womb had become so firm as to be competent to afford an adequate resistance to the impulsion of the blood. We can scarcely be induced to admit that the uterine arteries of every woman would at this age be uniformly and equally so firm or so diminished in their capacity as to present an insurmountable barrier to the current of blood. The thing is far from being probable. This cessation is in my estimation, deducible from some other source. That is, at this period, the ovaries lose altogether their disposition to fecundity. For nature, wise in her provisions, has by an unchangeable law ordered that the menstruous action should be discontinued, and the ovaries should lose their capacity to be impregnated, which served as an excitement to the womb, at that period when women could no longer afford nutriment to their offspring.

Of the end or ultimate purpose of the Menstruous Fluid.

To this day it remains a question, to what purposes this fluid is destined. Many believe that it is intended for the nutriment of the child. There were of the ancients as well as the moderns who countenanced this opinion, and for the reason that during gestation the menstruous fluid is discontinued, and was detained, as was imagined, as aliment to the embryo.

For some time subsequent to conception the fetus does not exceed, in volume, a bean or a wasp. And during this time can require but a small quantity of nourishment. What now becomes of the superfluous quantity? If the menstruous fluid be intended for this purpose, in the earlier stage of gestation it would be too abundant; in the later insufficient.* Added to this there are animals not provided with this fluid, but yet their young do not want aliment. Why therefore would writers look to this as nourishment for the child when it is obvious that the

* The whole of the menstruous fluid for nine months, the term of gestation, would not amount to more than five or at most six pounds; a child, at full time, together with the secundine, weighs from eight to ten or twelve, or fifteen, or even twenty pounds. Hence it is irrefutable that the menstruous fluid cannot be intended for fetal organization. Nature could not act upon principles so inadequate as to have her means thus incommensurate to her ends.

young of the lower animals have no such resources? we need not be at much trouble to furnish the reason they have not looked out and explored the real and true cause of the menstrual operation.

Certain authors have believed the menstuous flood to be offensive and hurtful. In the Livitical law some commands are to be found on this head. But, as the thing appears to me, more on account of the uncleanness of that people, than from a regard to any thing unclean or hurtful in the fluid itself.

We therefore must, according to my conceptions, consider that nature in furnishing the menstuous fluid intends nothing farther than to present to us certain signs of the maturity of the uterine system. From this view we may explain why it is that the discharge ceases during pregnancy; that is because at this period the ovaries lose their capacity to be impregnated which serves to rouse the arteries. Hence it is that we never, except in a few instances and these cases of disease, find this discharge to continue while the woman is pregnant.

Before I come to a final close, I must present my thanks, grateful and sincere, to my friend and colleague Dr. John Shaaff, and to others, from whom I have received acts of friendship, for the many favours and civilities enjoyed by their courtesy.

Th' inhabitants of seas and skies shall change,
 And fish on shore, and stags in air shall range ;
 The banish'd Parthian dwell on Arab's brink,
 And the blue German shall the Tigris drink :
 Ere I, forsaking gratitude and truth,
 Forget their kindness to my rising youth.

When the author presented to, and sustained before the University of Glasgow, as his Inaugural Thesis, the foregoing little work. he contemplated adding to it a short account of his views of the office of the Placenta: but finding himself anticipated by the learned professor Jaffrey, who, he understood, had employed his able pen on that subject, he abandoned his intentions. Yet it may not, even at the present moment, be out of place to state briefly the opinion entertained at that period*, and which he now, after much examination and reflection, believes to be well founded.

The Placenta has generally been thought to be a mere medium of connexion, or of circulation between the mother and child. To this the author has many, and, as he apprehends, serious objections to offer. But passing by, for the present, all laboured discussion, he will only state that, to his views, the Placenta holds the rank and performs the office of a secretory organ, a species of Mamma, and that its function is the secretion and separation from the mother's blood, of the aliment and oxygen necessary and appropriate to the fetal wants and demands.

* 1793.

A
TREATISE
ON THE
Autumnal Endemial Epidemick
OF
TROPICAL CLIMATES,
COMMONLY CALLED THE
YELLOW FEVER;
CONTAINING ITS
ORIGIN, HISTORY, NATURE, AND CURE;
TOGETHER WITH
A few Reflections on the
PROXIMATE CAUSE OF DISEASES.

BY
JOHN B. DAVIDGE,
A. M. M. D.

ALTERAM PARTEM AUDI.

BALTIMORE : Printed by WILLIAM WARNER.

.....
1813.

INTRODUCTION.

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IT is not my intention in the following pages, to unfurl the banners of personal opposition. Dr. Rush professes truth to be the object of his inquiries—truth is the end of my researches. The expanded mind of Dr. Rush, receives more sincere and real pleasure in the perception of one truth, than in all the fulsome incense that could ascend from a thousand flatteries: His great mind, I hope, is closed to the poisonous nutriment of boyish vanity.

The object of my labours and studies, for several years past, has been, in a great measure, an inquiry into the nature and etiology of diseases. And although, not unfrequently, I could meet with the opinion, that contagious diseases did often originate in marsh exhalations, yet as this opinion was accompanied by suspicious circumstances and wanted the support of probability, I treated it more as the fondness for novelty and innovation, than as the candid result of experience. Nor should I at present attempt to arrest its prevalence, had it not found too able an abettor in the learned and elegant Rush. Writers, of little note, cannot give tone to opinions. Falcons do not feed on flies. Great names alone can introduce great errors.

Who among us would have imagined, that the human mind was nothing but a flux of ideas, had not David Hume given currency to this hypothesis? Or that thought was a peculiar configuration of the combination of material atoms, had not Dr. Priestly introduced it to our acquaintance? But, to come nearer home, who could have supposed that the water-spout, that huge rising of the ocean, was effected by a power of suction resident in the clouds, without the hint of Dr. Franklin.*

* The true and real cause of the water-spout is, a column of electrick fluid descends from the clouds; this electrick column rarefies the air to a given distance; the rarefaction of the air causes an unequal pressure on the water: the equal pressure being removed, the water immediately rises, in bulk corresponding to the space of the rarefied atmosphere. This column of water ascends in proportion to the levity or thinness of the air, sometimes to an amazing height; and in its ascension acquires a whirl occasioned by the circumambient air rushing to a focus to fill up the partial vacuum and restore to itself its former equilibrium.

There is a species of water-spout, which, it is said, at times occurs on land. This we imagine is produced by a condensation of the aqueous vapours, constituting the cloud, in consequence of a sudden loss of electrick fluid and elementary heat. Nearly upon the same principle, it is probable, as hail is formed in the hottest weather of summer. This phenomenon, to our understanding, is obviously produced by rapid evaporation occasioned by an almost instantaneous escape of a large quantity of electrick fluid, either from two clouds, the one plus and the other minus, meeting, or from a cloud highly charged passing within striking distance of some elevated district of land.

Great names have a species of magick accompanying them. Mr. John Hunter gave, for a time in London, tone to the doctrine of digestion. His opinion, that it is the combined action of friction and fermentation, has been successfully combated by being put in competition with one better founded. Spallanzani, by a series of well managed experiments, has exhausted the subject of all doubt. According to this indefatigable man, it is nothing more complex than a simple chymical process; such as takes place in the union of sugar and water, where the attraction of combination overcomes the affinity of aggregation: That which is proper nourishment is taken up, by the lacteals acting as capillary tubes, what remains is thrown off as excrementitious refuse.

Names frequently are more propitious to books than their contents are. And many works, of sterling value, too often instruct none but their authors.

This comes forth, neither connected with name, nor clothed with title; I leave it to recommend itself. If it prosper, well; if not, it will only, suffering the mishap which overtook one of David Hume's best productions, fall dead-born from the press.*

* When the above was written the author did not intend to have affixed his name to it, but for particular reasons he changed his determination.

The reader will bear in mind that the following few imperfect pages constitute the first english production in which the author's pen was ever engaged*: that he was not at liberty, to imagine himself employed in merely dressing a brilliant period of polite literature, or polishing a trope in rhetorick, but seriously occupied in disquisitions of a much higher, and a more important nature; and also recollect, that, it was written a few months after the author had taken on himself the discharge of the highly responsible duties of his profession. Keeping in sight those points, the Reader, it is hoped, will generously touch with a light hand, and view with a pardoning eye, the many inaccuracies and the juvenility of the style.

* It was first published in 1798.

TREATISE, &c.

CHAPTER I.

THE ORIGIN, HISTORY, AND NATURE OF THE YELLOW FEVER.

THE yellow fever, synonymous with la Maladie de siam, or la Fevre matelotte of the French, and vomito prieto of the Spaniards, is the majority or acmé, as the intermittent is the infancy, of the remittent bilious fever. It is to the common bilious fever, what the confluent, is to the common mild, small-pox. They are in kind the same, a specifick difference only exists between them. It is conceived in the same matrix, and quickened by the same sun.* It is indigenous to America, and all other warm climates. It is the great outlet, to Americans and Britons, from life to the grave.

The rays of the sun, diffused and scattered, are innocent and salutary, but collected and condensed into a focus are dangerous and hurtful.

* "Without the matrix of putrid vegetable matters, there can no more be a bilious or yellow fever generated amongst us, than there can be vegetation without earth, water, or air." Rush vol. 3, p. 168.

Thus the concentrated effluvia of marshes are venomous and deadly ; but scattered and diffused, float innocently among us.

The generality of the French writers call it *la maladie de Siam* from a false notion that it was originally from Siam, a country in the east. This contains as much truth as the opinion that it attacks sailors only: whence they call it *la fièvre matelotte*. It is, in all probability, the *Causus* or *Tebriis ardens* of Hippocrates, of Aretæus and of Galen. Trallian and Lommius appear to have seen this fever.

Ulloa makes mention of the vomito prieto prevailing, in a most horrible and destructive form, in Carthagera, in the year 1729 and 1730*. It made its inroads in Barbadoes in the year 1696† a time long prior to the visit of Dr. Warren to that island.

Marsh effluvia appear to be either the decomposition of vegetables or water; but whether inscrutably combined with something else, or insulated, I leave for farther investigation. In the decomposition of either, hydrogen is produced in considerable quantity. Water, in its liquid state, is a compound of hydrogen and oxygen, with an addition of caloric and a little

* Voyage to South America, B. 1, C. 5.

† Hughs' History.

common air. Its decomposition is affected by the matter of heat inserting itself between the liquid pherules until their separation is such as to annihilate the attraction of combination between the hydrogene and oxygene. In effecting this decomposition, however, there must be a third agent. Whether then an union, betwixt the oxygene and light, comes to pass, remains to be discovered: this I strongly suspect. The hydrogene being disengaged and insulated, and very much accumulated, or peculiarly combined, with some subtle poison, perhaps becomes the peccant agent.

It is worthy of observation that the bilious, or yellow fever does not generally prevail during the heat of the summer. This may be owing to the greatness of the heat dissipating and scattering the hydrogene, or marsh effluvia, so as to enfeeble and render it or those innocent.

That a combination between the oxygene and light happens is likely, first from their natural affinity to each other, and secondly from a phenomenon observable during ignition. The rays of light falling immediately upon the ignited combustibles, cause the flame to become faint, and ultimately will extinguish every particle of the fire. The probability is, that this phenomenon is occasioned by the rays of light attracting and combining with the oxygene of the atmosphere, and thereby interrupting the process

which was going forward between the oxygene and combustible bodies. To this there is an accession of additional strength from what takes place in vegetation.

A vegetable, kept in the shade, becomes white, and sickly: when it is exposed to the light it revives, and becomes healthy. This I apprehend arises from the light acting as a stimulus, and at the same time attracting from it its oxygene, with which it is necessarily charged in decomposing water for its nutrition. Vegetables when analyzed yield more or less of hydrogen. Hydrogen is that gas which in its struggle to ascend, meets with the electric fluid of the atmosphere and forms what, in vernacular vulgarism, is called jack-o-lantern—or when it has gained the superior regions and formed the upper strata of the air, comes into contact with the electric fluid, and effects what is indicated under the appellation of *auro-ra borealis* or northern lights*. It is also the principal agent in the motion of the aerostatic machines.

The yellow fever made its first appearance in the city of Baltimore in the last of August. The common bilious fever prevailed at the Point from June. A lady from Philadelphia, bring-

* It is presumable, that, in those meteorous phenomena, phosphorus may be more or less concerned.

ing with her the seeds of the disease, which were brought into action by the fatigue of the journey, was severely attacked with it in Charles-street, she had the genuine black vomiting, which resembled ink and coffee-grounds mixed, for two nights and a day, and miscarried on the sixth night of the disease.—She notwithstanding recovered. No person in the family, or neighbourhood had it during the whole season.

This, together with the number of cases of violent bilious fever at Fell's Point, threw the city, generally, into a combustion. The committee of health requested that the physicians would convene in order to give a report of the city; upon the meeting of the physicians, it appeared, from their joint testimony, that the above-mentioned lady was the only person, labouring under the fever, in the west end of the city, called the town in contradistinction to the Point. [I shall in what follows make use of the distinction of town and Point.] The committee received a letter from Dr. Coulter, a physician of great respectability at the Point. Dr. Allender, in person, waited on the committee, and gave information of that part of the city. It was requested by the committee of health, that some of the physicians of the town would visit the Point. Pursuantly to this request, doctors Goodwin, Moores, and Davidge politely went, and waited on the sick individually; they were piloted by a gentleman who lives with Dr. Al-

lender, together with a student of Dr. Coulter's. Their report was; "that there was nothing more than a violent remittent bilious fever prevailing." This perfectly accorded with the sentiment which dropt from one of those physicians during his report to the mayor; "his reason (he said) why he did not report the case of the lady of Philadelphia, was a full persuasion, that the yellow fever, being of the same origin with a bilious fever, could not be multiplied, by an intercourse of bodies, under any circumstances whatever."

The above report is a strong and prominent feature of their discernment, and indifference to popular prejudices. A little after this period, the disease, rising from the rank of a bilious, to that of the yellow fever, mounted its chariot of death, and spread dismay and mourning wherever it approached. Conveyed by the north-east wind, it diffused itself all along Federal-hill, and west end of the bason. Whichever direction the miasmata (arising from the stagnant water and marshes about the Point and wharves) controled by the winds, took, the disease closely pursued. It evolved, in an horrid and dismal manner, its venemous characters in the south end of Hanover-street, and its vicinity. After a short interval these deadly effluvia penetrated into the centre of the city, and many who were not near the Point nor wharves, those exuberant fountains of mischief, were diseased

either in their houses, or in the streets. Not an inconsiderable number of those who were at the launching of the frigate took the disease; several of whom died of it afterwards up in the town; but, fortunately for the citizens, no person was infected from them. A very considerable part of the Point fled into the country; and some from the town removed; a temporary desert was effectuated.

I scarcely need mention the daily deaths; the reports of the committee of health are inadmissible of an accession of testimony, to give weight to their authenticity and accuracy.

The greatest number, in the town or west end of the city, in any one day, was seven or eight; the list of the deaths at the Point was, for some time, considerable. This fever began to cease about the first of October, and was nearly or quite extinct on the first of November.

All endemical epidemics, as they depend on a peculiar constitution of the air, must of necessity cease whenever this condition is destroyed, whether it be by frosts, or rains.

The yellow fever, like all other epidemics, delights in solitude. An epidemic, whether endemical or contagious, depends on a general peculiarity of the air;* which general peculiarity

* This peculiarity of air is attributable in the one instance to a general diffusion of vegetable, and, in the other, of animal effluvia.

will have a general influence within its own dominion, and communicate a general aptitude to all bodies, within this jurisdiction, to receive its action. It chases away all others of less strength, as is justly observed, first by Diemerbrook, secondly by the Sydenham, thirdly by Pouppé Desportes, and after them by Rush; who is perfectly correct, where he says, that no two epidemics, of unequal force, can prevail at the same time. How is it possible for two general and opposite constitutions of the air to exist at the same time? with equal propriety we would say that a cord can possess two distinct oscillations at the same instant; or that two particles of matter can occupy the same given space in the same division of time. But this every tyro should know.

Some of the first traces of the yellow fever, in America, are to be found about sixty or seventy years back. A physician, in conversation, the other day told me that he had met with the yellow fever, in Baltimore ever since he had lived in it, which is fifteen or twenty years. It is violating all obligations of decency and truth to say that it is of recent date. The town of Baltimore, in proportion to its inhabitants, is less subject to this autumnal remittent or yellow fever, than the low situations about the Potowmack. A gentleman, who for some considerable time was one of the principal directors of the Potowmack business, informed me that one season they lost a considerable number of their

workmen by the above fever. And that in no instance did it spread by contagion.

Every country has diseases proper to its climate and situation. Some diseases are common to every country and climate : Accident and particular circumstances will create sporadick diseases, in every country, not peculiar to them respectively. A disease, proper to one country, can, by the medium of intercourse, be carried into another and there propagated.

Britain has its scrofula, and typhus ; the vicinity of the Alps has its goiter : the East has its plague ; the West-Indies, America and other countries, within or near the tropicks, have their remittent bilious fever, and hepatick affections. It is not the import of this paragraph, that those diseases are exclusively generated in these countries respectively. The reverse is incontrovertible. But as those complaints, although they may originate, or be produced, in every country and under every climate, most commonly and generally appear in those countries in the manner above-mentioned, I have taken the liberty to style them proper to those individual climates. It is a concurrence of circumstances, and not given latitudes, that is requisite for the production of diseases.

The seeds of the remittent bilious yellow fever are produced in putrid vegetables and stagnant water, and are quickened by heat and dry-

ness. Those who live in the neighbourhood of marshes, in warm climates, and during hot and dry autumns generally suffer more or less. The more elevated situations, and those distant from such sources in the West-Indies and America, most generally escape this deathful malady, except when the atmosphere becomes universally surcharged with the effluvia emanating from those exuberant fountains of mischief and poison ; under such occurrences they unavoidably participate of the evil. This is confirmed by the united voices of writers and practitioners.

Males, by being more exposed to these effluvia floating in the air, to the violent rays of the sun, to night-dews, and to the various, sudden, and great vicissitudes of the weather, (these operate differently according to their respective natures) are more subject to this, and all endemical epidemics, than females, whose business is naturally within their houses, where the poison is blunted and rendered inert by the fires and smoke, and has itself dissipated by striking against buildings.

The skirts of most cities are occupied by the poorer class of inhabitants ; their houses are exposed to the first and most violent assaults of all endemical epidemics ; but as those epidemics invade more the vitals and heart of a city, they are dissipated, enfeebled, and disarmed.

The remittent bilious yellow fever neither pities helpless infancy, nor reverences the decrepitude of age ; but its chief delight is to jostle, from the stage of life, the young and vigorous. The many-headed monster, armed with destruction and wo, wantons in the citadel of life : and now, Anthrophophagi like, without warning or premonition, implants his merciless dagger ; and now under the garb of innocence, gambols in the blushing cheek of health. The yellow fever is as fatal when with the investiture of an intermittent, as when habited in its regular dress. Having explored its cause and origin, we may trace out its nature.

Dr. Rush.

“ The fevers generated by putrid cabbage, mentioned by Dr. Rogers, and by putrid flax mentioned by Dr. Zimmerman, were both contagious. Dr. Lind ascribes the yellow fever every where to marsh or vegetable exhalations ; and this fever, we know spreads by contagion. Dr. Lind, jun. establishes the contagious nature of the marsh fever which prevailed in Bengal in the year 1762. I shall transcribe his words upon this subject. “ Although marsh miasinata (says he) first bring on the disease, yet contagion presently spreads it, and renders it more epidemick. Thus the Drake Indian continued free from the disorder for two weeks together, when she had no commu-

The Author.

An endemick is a disease, that afflicts several people together in the same country where it reigns, arising from local circumstances, or a peculiar condition of the air. It cannot be carried from one country to another, by the means of bodies and cloths. It affects more or less all within its own periphery. It cannot be carried, beyond its own atmosphere, by bodies diseased with it. No disease, arising from marsh effluvia, can be communicated beyond the atmosphere charged with these effluvia. Every disease arising from marsh effluvia, from the gentle intermittent to the furious yellow fever inclusive, then is endemick.

It does not appear, that any of the cases, in the opposite

nication with the other ships ; whereas as soon as the disorder was brought on board, many were seized with it within a few days in such a manner as to leave no room to entertain the least doubt concerning its pestilential nature."

Dr. Clark mentions a contagious malignant fever from marsh miasmata, which prevailed at Prince's Island in the year 1771, and which afterwards infected the Grenville Indian. The contagious pestilential fever in France, so accurately described by Reverius, was produced by an exhalation from putrid vegetables, particularly hemp and flax. Even intermittents, the most frequent and most numerous offspring of the marsh exhalation, are contagious. Of this there are many proofs in practical authors. Blanchi describes an intermittent which was highly contagious at Wolfenbuttle in the year 1666. Dr. Clark mentions a number of cases in which this mild species of fever was propagated by contagion.

Dr. Cleghorn has established the contagious nature of intermittents by many facts. After mentioning numerous instances of their having spread in this way, he says, "These tertians have as good a right to be called contagious as the measles, small-pox, or any other disease." &c. *Vol. 3, p. 160.*

column, communicated themselves by means of the diseased bodies beyond the circumference of the atmosphere impregnated by the vegetable exhalations ; then if they did not, which is pretty obvious, act beyond the limits of the inquinat-ed atmosphere, the probability is, that the vegetable or marsh effluvia wafted through the air did the mischief, and not the intercourse of bodies.

That a ship's crew was free from a disease this week, is no just argument, if they be infected next, that it must be by means of one diseased body communicating it to another ; the contaminated air may have enlarged its limits—a particular direction of the wind may have conveyed the bad air to the ship. It happening posteriorly to a communication with the diseased, is no argument, except at the same time it be proved that this crew was without the control of the ill-conditioned air. Marsh and vegetable effluvia smite at the distance of miles, this no physician doubts.

"These diseases (speaking of intermittents) make their first appearance in February and August particularly ; tho' sometimes they appear sooner or later, according as the air is more or less disposed to produce them, which, of course renders them more or less epidemic." P. 51, Dr. Sydenham

A question of importance and magnitude here arises and invites our attention ; it may serve to awaken a little our judgments and elucidate the present stage of the business. Why is it that the intermittent fever has never clothed itself in its contagious habiliment in America. It is rather problematical that it has ever done more mischief, or been more common and violent in any other country than in America. Here is a mystery equally dark on all sides ; how are we to decipher the enigma ?

“ *Intermittentes.*

Febres, miasmate paludum ortae, paroxysmis pluribus, apyrexia, saltem remissione evidente interposita, cum exacerbatione notabili, et plerumque cum horrore redeuntibus, constantes : paroxysmo quovis die unico tantum.” Dr. Cullen, Tom. 2, Synop.

Dr. Cullen, the luminary of the medical world, has been careful to inform us, that he has never met with contagious diseases arising from vegetable putrefaction.

Sydenham, styled by Dr. Rush, “the incomparable physician,” positively asserts that an intermittent becomes epidemick from the *air*, and not by contagion. Dr. Jackson says that the intermittents and remittents become epidemick and spread by the marsh miasma being dif-

fused through the atmosphere, and not by contagion (vide Jackson on the diseases of America). Dr. Gilchrist mentions in his tracts on sea voyages, that the marsh effluvia being scattered through the air become the source of popular diseases. That diseases, arising from marsh miasmata, are incapable of becoming contagious has been considered, by the generality of physicians, as a fact almost self-evident. And I believe that few will long hesitate to determine between the authority of Sydenham and Cleg-horn.

The unscientifick notion of intermittent and remittent bilious fevers being contagious, is too ridiculuous to find access even to the easy credulity of the unread peasant: but yet it is embraced by philosophick refinement. And what absurdity, though ever so enormous, has not, at one time or another, been countenanced by proud, arrogant, fastidious philosophy? Organized absurdities, have long conspired against the progress of science, and tyrvanized over the tender elaims of more luminous systems.

Endemick is the antithesis of contagious. Endemick and contagious establish two opposite categories.

Contagion is the emission, from body to body, by which diseases are communicated. A contagious disease arises originally from human

effluvia, can be carried, by means of the infected bodies, or cloths, from country to country, from city to city, from town to town; in fine, from any one place to another: The yellow fever has not its origin from human effluvia, cannot be spread, by means of diseased bodies, or wearing apparel, or bed-cloaths, from country to country, from city to city, from town to town, nor from one place to another—the yellow fever therefore is not contagious.

Either an endemick or contagious disease, becoming general, and affecting a whole country, or great extent of territory, claims, in technical language, the appellation of epidemick. It is not whether a disease arises from vegetable or human effluvia, but whether it has a general or universal action, that constitutes it an epidemick. It is the universality of its action, and not the nature of the source whence it arose, that stamps its character. Hence two classes of epidemics—the endemial epidemick, and the contagious epidemick. The yellow fever ranges under the former, the plague under the latter.*

* So far as accessible authority will justify a conclusion, the author is the first writer of America, who has publicly advanced the opinion that the Yellow Fever is not a contagious disease. And he would feel no inconsiderable degree of pleasure in perceiving many of the most impassioned opposers of his sentiment among its warmest admirers, were it not, that these learned gentlemen, with his Treatise in their hands, pretend and assert (with how much justice may be left to the reader), that the discovery is the *result of their own observations*.

Dr. Rush.

"It has been remarked that this fever did not spread in the country, when carried there by persons who were infected, and who afterwards died with it. This I conceive was occasioned, in part by the contagion being deprived of the aid of miasma-ta from the putrid matter which first produced it in the city, and in part by its being diluted, and thereby being weakened by the pure air of the country. During four times in which it prevailed in Charleston, in no one instance, according to Dr. Lining, was it propagated in any other part of the state." Vol. 3, page 157.

The Author.

When the intercourse of bodies, labouring under a fever, cannot support and multiply the said fever, the fever is not contagious; the yellow fever cannot simply and without the addition of the effluvia arising from vegetable putrefaction support and multiply itself, the yellow fever therefore is not contagious.

A little attention to the nature and operation of the small-pox, measles, or jail-fever, when conveyed into the purest atmosphere, will, in some measure, obviate the difficulty which obscures the above fact in relation to the yellow fever. It is found upon experiment, that, uniformly the small-pox, or measles, or jail-fever, requires nothing more than its own presence and virulence to perpetuate itself. This is clearly apparent from the many melancholy instances of whole families being precipitated to their graves by the unfortunate introduction of servants, purchased from on board ships, infected with the typhus or jail-fever. Out of fifty persons, who might visit a patient labouring under the latter stage of the small-pox or measles, for

ty-nine, in all probability would be infected. I have selected those diseases, as they are common and known to every body.

In these diseases, which are essentially and absolutely contagious, no heterogeneous aid is necessary. In the yellow fever the addition of the marsh miasmata seems to be the *sine qua non*, in multiplying and propagating the complaint, and the efficient, occasional cause of its rise, independently of any assistance from bodies : and without the help of these miasmata, the bodies can neither generate nor spread the disease ; hence it appears that the miasma produces the disease whenever and wherever it may occur, and not the bodies labouring under the complaint.

It is not my expectation to turn, upon the axis of a syllogism, the whole esculapian world. Prejudice may for a time bar the understandings, but time and accident will eventually unlock the judgments of men.

Dr. Rush.

“ Let it not be inferred from the enumeration of the means of preventing the contagion of this fever, that I admit a contagious nature to be one of its characteristick marks. Far from it. It is an accidental circumstance produced chiefly by the concurrence of the weather.” And on the same page
“ It is in no instance contagi-

The Author.

A disease which is not essentially, and in its own nature contagious, is not, in strict propriety, contagious at all. The yellow fever is not essentially, and in its nature contagious—the yellow fever is therefore not contagious at all. A disease, which is not intrinsically and in its very nature, radically and inherently contagious,

ous in some cases." Vol. 4, | naturally repudiates the idea
page 61.* | of contagion. Fire, elementa-
| rily, contains heat; and light
| is, intrinsically, luminous.

If the vegetable effluvia do, and can, of themselves, produce, perpetuate, and multiply the yellow fever: and if the human effluvia cannot and do not in any instance, of themselves, produce, perpetuate, and multiply this fever, upon their fortuitous union; which can be supposed to effect the mischief? The utmost precipitancy cannot endanger the decision.

The bodies diseased certainly produce an effluvia, which, by disturbing directly or indirectly the bowels, stomach, or sensorium commune, may destroy the equipoise of the animal system, and thus prove an exciting cause, in like manner with drunkenness or night-exposure; hence the aptitude of those who have to nurse, and wait on the sick in the yellow fever, to be diseased. This is not by receiving it by way of contagion: but under those circumstances the equilibrium of the body is destroyed, the œconomy is thrown into disarray, the vigour is unnerved, and an advantage is afforded the miasma to bring itself, with all its deathful consequences, into full operation.

The government of a country is called good or bad, according to its nature; the best govern-

* This paragraph, for its interpretation, requires something above the author's comprehension.

ment becomes hateful and detestable from bad administration ; but the administration is not physically or morally consecutive of the government. In this instance we are to depose our minister, not change our civil system. Thus if we remove the patient, in the yellow fever, from the iniquated atmosphere, he is no longer dangerous or hurtful to the attendants.

The christian system, the substratum of hope and felicity, has become hostile to the peace and safety of nations, by its administration, not by its nature, its essence is peace, and life, and immortality.

Many things, in their nature, elude the most vigorous effort of the human intellect, and tantalize the grasp of genius itself. They present to us their modes, and qualities, and affections ; these, operated on by the instrumentality of the senses, advertise us of their respective existence. It is from the effects of human, and marsh effluvia, that we can have any clue to the secret of their nature. Like causes will always produce like effects, provided they operate upon the same order of patients. The human effluvia produce one order of diseases, the marsh effluvia another, these orders never unite—they have no connection. Of these identity forms the curve, and those effluvia the asymptotes, they apparently approximate, but they can never come into contact. The human effluvia can

never produce the remittent bilious or yellow fever, nor can the marsh effluvia ever give origin to the typhus or jail fever.

Is it good logick, that a fever can arise from one source to-day, and from another, diametrically and physically opposite, to-morrow? This is certainly at variance with common ratiocination. Yet this must come to pass, if the yellow fever, originally, has its origin from vegetable putrefaction, and is afterwards perpetuated by contagion. Some have asserted that heat and cold, though different, produce the same effects. Cold is a relative term—it has no absolute existence—and that which has not an absolute existence cannot possess a positive action.

Sir Isaac Newton, who was a man of no humble genius, is of the opinion, that it is incompatible with the principle of philosophizing, in the explication of any phænomenon, to adduce more causes than are true, and absolutely necessary for its solution.

After the above theoretical observations, we shall direct our attention, a little, to the authority of experience, and minute inquiry, of some of the most respectable writers and practitioners.

It is an unquestionable truth; a verity of the most publick notoriety, that not one, of the very great numbers who have left the cities and towns, some of whom have died and some have

recovered, has communicated the yellow fever to those who have attended. Not one solitary fact has ever reached me ; and my scepticism is such as to lead me into a persuasion, that there has not existed one unequivocal, well analyzed fact of a patient, going to the country, and there multiplying this fever.

“ It affects the inhabitants of cities, and not of the country, as in Charleston in the years 1732, 1739, 1745 and 1748. And in Philadelphia in the year 1793.” That is, it could not be propagated, by the diseased bodies, in the country. But that it can affect inhabitants of the country and also originate in the country is fully defensible. I know that my opinions here, as in other parts of this essay, are the antipodes of those generally received. But while I have facts, and those of the most stern and inflexible nature, I shall not be over solicitous about received whims whether popular or professional.

On the Potowmack Bottoms, and along the Monokocy, I have seen the most unequivocal cases. It is also, from the very respectable authority of Dr Watkins, very common in different parts of Kentucky. Dr. Watkins was in Baltimore during the prevalence of the fever this autumn, and, with me, waited on the sick. He, after being provided of all necessary requisites to form an opinion, declared it to be precisely similar to that of Kentucky. He farther

observed that the fever in that state frequently was attended by the black vomiting, &c. He said that it was considered there as an endemick arising from the marshes, and in no instance contagious.

It is true that Dr. Rush mentions an instance of this fever spreading in the country; but we want graver authority than inaugural theses afford. Theses, we all know, generally, are nothing but the echo of the prelections of a preceptor.

In the flourishing and growing city of Baltimore, we have had the most stubborn, and irrefragable proofs, of the yellow fever being incapable of supporting itself, in the cases which have occurred about the wharves and Fell's Point. After those cases were removed up into the city, they had their virulence to die with them, those who died; and from those, who recovered, all mischief and supposed contagion evanescenced into the empty air, which bore it to the pages of medical writers, not to the bodies of healthy attendants. This was the result in 1794 and 1797.

The unfortunate case, of the very respectable Dr. E. Johnson, with several cotemporary incidents, afforded a short-lived triumph to those who were wedded to the contagious system; but when their opinions, armed with all the address and subtlety of the authors, came in collision, with those of more erect and manly aspect,

they felt their vacillating uncertainty, and ceded in candid contest.

Which is most consonant with probability, for gentlemen, going to an atmosphere, charged with poison and deadly effluvia, an atmosphere immediately wafted from the Point and concentrated under a hill, where many had been forced, by its deathful influence, to pass the bourn of life, and others to perceive death, with its black wings, to hover about them, to take it from those exanimous and dying bodies, or this noxious air? Taking into view, at the same time, the fact, that none of those gentlemen, several of whom died up in the city, communicated, to any of their families or attendants, the disease.

Some even point out the luckless moment, in which the relentless malady seized them. "At a particular juncture," says one, "I perceived something singularly offensive—in three hours after that inauspicious point of time, I became unwell." Another was taken ill a day or two posteriorly to some ill-omened hour, in which the breath of the patient was breathed directly into his face; it was then he inhaled, with the pabulum of life, the fermenting leaven of death. Who, after these and such like melancholy tales, can hesitate to believe the yellow fever contagious?

I would no longer quarrel with the sentiment of contagion, had one of the above occurrences

taken place, in people, who were not, at the time of the imagined infection, in an impure atmosphere, or had not lately been exposed to air impregnated with the destructive miasmata. The fact is, those persons were all in this ill-conditioned air. I met with two cases, in two young men, who had been at the Point at the same time, and were both attacked, on the same day, with all the regular symptoms of the fever, where the miasmata did not come into action until the eighteenth day—they were not, during the interval, exposed to either sick bodies, or vegetable effluvia, having been in a healthy country the whole time.

Whatever affects most our senses, we are prone to attribute our evils to: This is excusable in men unaccustomed to thought, but it is an incongruity in the common order of reasoning, and is at total variance with the notions of men of science.

Logomachy is as much at variance with my habits, as any two opposites in nature are inconsistent with each other. But I do not consider a war of sentiments, fraught with mischief or good, a colluctation of principles, tending either to support or destroy the commerce of a nation, as the idle jar of words. It is no new act for men to force the most opposite and contending principles into the most cruel and unnatural union. Tradition, let it militate ever so much with com-

mon sense ; and an association of ideas, bearing no affinity nor cognation to each other, press us, too frequently, into most absurd beliefs and habits of thinking. I might here cause to pass in review before the reader's mind the great Lavoisier's philosophy of faces, that finished and polished physiognomy of folly. And where is a philosopher, bating a few, who does not stuff his works with the infinite divisibility of matter ? who has dared to dispute the corollary, of the great Locke, that the human mind is, at first, as a blank sheet of paper, passive to the characters of chance ? and where are the friends of David Hume, who do not, to this day, believe, that, if an ass were placed between two cocks of hay, which impressed equally, he would starve, being unable to make a choice ? But I am straying : it may be well to obey the suggestion, that we should tread lightly on the ashes of the dead ; I bow obedience, and may David rest in peace, and his errors sleep in eternal silence.

Now were I to plunge into the vacuum of metaphysics, or enter the lists in pneumatology ; I should believe, with the peerless Reid of Glasgow, that the human mind possessed, inherently, action, vigour and choice ; that it operated upon surrounding objects, and was not the passive sport of incidental impression. Whence comes genius, the innate perspicacity of the human intellect ; genius, that heavenly offspring,

at all times impatient of the trammels of control, and indifferent to the habits of education? not from the impressions of the beauties of Thompson, nor the more sublime of Homer. It is in-born. Did the mountains of Switzerland, give exertion to the mind of a Haller, or the banks of the Potowmack, infuse divinity into a Washington? that encyclopedia of virtue and greatness, in whom may be found every ornament of human excellence; into him who first boldly dared, to rend asunder the strong ligaments of prejudice, to control the imperious tide of ancient usage; who magnanimously despised the boisterous torrent of vulgar obloquy, and challenged the herald of recording time?

This apparent digression is to shew, that no name is above truth, that our care and solicitude should be the investigation and conservation of truth, not the support and protection of names and traditions.

Doctor Rush.

"An aptitude or predisposition from season, climate or constitution must concur to render the contagion of this, as well as other malignant fevers, sufficiently active to produce disease; as well might a traveller attempt to describe the climate of a new country, from the history of a single season, as a physician to fix the character of an epidemick from its appearance in one season, or one country." Vol. 4, page 62.

The Author.

"The one (the yellow fever) is evidently caused by marsh effluvia, heat, violent exercise in that heat, thick, hot, moist atmosphere; night air and dews, and the abuse of spiritous liquors. The other (the boudin fever) on the contrary, is caused by contagion alone. This is certainly the most remarkable difference; and constitutes an obvious, clear, and indisputable diagnosis." Chisholm, page 147.

“ But I never could observe any one instance, where I could say that one person was infected by, or received the fever, (yellow fever) from another person who had it.” Hillary, page 145.

Dr. Jackson calls the yellow fever of Jamaica an endemick, and no where mentions its being contagious ; who also is of the opinion that most epidemicks spread by means of the *marsh effluvia*, and not by contagion.

Dr. Mosely styles it (the yellow fever) an endemial causus, page 391. And ridicules the idea of its being malignant, pestilential, and contagious, as asserted by Dr. Warren, page 412. Dr. Warren, as Mosely justly observes, had scarcely any idea of this fever at all, except in its description. Towne calls it *febris ardens biliosa*, but does not add *contagiosa*, and asserts it to be an endemick of the West-Indies.

That it appeared, at Barbadoes, anterior to the time (1725) fixed by Dr. Warren is evident from Dr. Gamble, who well remembered it to be very fatal, in the island, in the year 1691. That Warren’s account of the disease is altogether fabulous is certain : this man, wrapt in hypothetical clouds, was carried about by every wind of error.

Pouppé Desportes speaking of the *la maladie de Siam*, says ; “ *la régularité avec laquelle elle se reproduit, semble devoir la faire regarder*

comme une de ces maladies dont il faut chercher la cause dans la *constitution de l'air*." Page 191, Tom. 1.

Dr. Cullen supposed this disease to arise from human effluvia and therefore, placing it under the section of contagious diseases, called it typhus Icterodes—but it is certain Dr. Cullen never saw the disease, and equally certain that he borrowed his idea from Dr. Warren—Dr. Cullen is consequently to be left out of the present discussion.

As well might you contend, that a skilful gardener could make a tree flourish in a soil unnatural to its growth, as physicians ingraft contagion on marsh exhalations.

In the almost endless chain of cases, wherein the clothes of those who have died, in the West-Indies, from the yellow fever, were brought to America, contagion would be explored in but one instance, even by the indefatigable industry of the sagacious Rush. A gentleman's clothes being returned in a trunk to his friends; a young gentleman, upon opening the trunk, became immediately unwell, but no other person suffered in the least. Those clothes, beyond a question, were damp, this dampness was by heat converted into effluvia, corresponding in every particular with marsh miasmata. These effluvia, from the closeness of the trunk not be-

ing able to escape, remained in their offensive and concentrated state : And, upon the trunk being opened, applied themselves in their full force to the excitable system of the young man. In this manner, from the water in the clothes being changed into effluvia similar to those of marshes, and not from the clothes acting as the vehicle of the human effluvia, was this gentleman diseased. Now if the supposed contagion was so very vivacious as to deproduce itself after so long a lapse of time, how is it that the recent emanations from a diseased body treated with such great tenderness the attending friends, as not to interfere in the smallest degree with their healths ?

The idea of contagion is indirectly injurious to commerce, and directly to society. Under the influence of the persuasion, that there is one of the most violent of contagious diseases prevailing in several of the sea-ports of America, can we suppose that foreign ports will suffer our vessels to enter ? A long and dangerous quarantine must be performed ; the damages, accruing from such delay, the merchants too sensibly feel to be ignorant of. Even when our papers do not proclaim the melancholy tidings ; seeing that such disease has frequently visited our cities, is it not probable that foreigners will guaranty their own safety, by prohibiting the entrance of our vessels ? will they not naturally say ? the Americans have seen their

error in rendering publick their diseases—their silence is a piece of policy—the diseases exist : interest, the main spring of human action, forbids their publicity.

From slender causes great events come to pass. Few could have supposed that the interest France took in the American struggle would have laid the corner stone of her ruin and overthrow ?—The government I speak of. Athens was ruined solely by Pericles giving countenance and support to a despicable tribe of stage-actors. Upon a slender pivot play weighty matters.

Our extraneous interruptions are not the only inconveniences we labour under, domestick commerce is subject to arrestation, a general calm and stagnation in business depresses our prosperity.

Has it ever been known that the yellow fever has been propagated through Baltimore, or any other city, from a person bringing from Philadelphia this malady, even in instances where it has proven fatal ? The archives of America will never record such a fact.

Do not the grave proclamations, and serious resolves of city-corporations excite our astonishment ? What are these proclamations and resolves for ? To prevent, what, from the earliest dawn of lapsing ages, has never, and to the latest eve of expiring time will never, come to pass.

Our interest and commerce fall a sacrifice, and are immolated at the shrine of our folly. As long as my reminiscence will recover to me a knowledge of the proceedings of the health-committee of the city of Baltimore, I shall admire and esteem their judgment and good sense.* Has the yellow fever ever been imported, from the West-Indies to America? Report says it has; but where are proofs? smothered in impenetrable obscurity; they fly the presence and converse of investigation. Has this fever ever been carried from the West-Indies to Britain?†

* This, with several other passages refer to the circumstances of the time, and cannot be understood but by those who may be possessed of a knowledge of those circumstances.

† On this subject Dr. Wilson in his valuable work on febrile diseases, has furnished us with a most important fact. "Dr. Lind, remarks he, mentions a fact, for which it is still more difficult to account, that the same fever brought into this country in several American ships, *attacked those only who had been on board the ships*, others remaining uninfected, notwithstanding the *freest intercourse* with the sick on shore." Vol. 1, page 178.

It is not a little surprising that a man of understanding and science should be at a loss to account for this fact. If we were to admit the ships to be foul and highly charged with putrid vegetable effluvia, might we not thereby be provided with a clue to the explanation of this mystery?

The persons who went on board the ships came into contact with the poisonous effluvia which at first produced the disease; those who remained on shore were exposed only to the human effluvia issuing from the sick and the dying.

The conclusion here is inevitable; the disease does not spread by contagion, but is produced by putrid vegetable matter only.

The poor unfortunate subjects of disease, flying from the cities, find the doors and windows of the country barred against them. The children leave their parents to die, and parents their children, their minds being jostled by the sound of contagion, from their proper seats. The lonely hearse solemnly conveys the dead to the dreary repository of the sleeping multitude, where reign silence and death. Having considered the history and nature of the yellow fever, we shall pass on to a consideration of its symptoms, first premising a few thoughts on the proximate cause.

CHAPTER II.

Proximate cause Analyzed.

THE fabrick of the pathology of diseases, has for more than two thousand years floated on the varying ocean of incertitude, the sport of winds and tide. When the microscopick eye traverses the hemisphere of medicine, it beholds theories hurled on theories, fancies crushed by fancies, and less errors smothered by those of greater bulk and effrontery. From the auspicious days of Hippocrates, we gently glide down the silent tide of time, collecting as we move the shattered wrecks of crazy systems, until we arrive at the fluctuating variety of modern hypotheses. Hippocrates, wrapt up in the flattering pretensions of his humoral pathology, and balancing between heat and bile, a long time swayed the sceptre of the medical world. From an attentive perusal of this author's works, heat or bile, or plethora or obstruction (for in different passages he speaks of all these) seem to constitute the proximate cause of fever. His successors soon perceived the futility of this foundation, and attempted to fabricate others more probable.

Diocles of Carystus, a physician, who flourished at an early period, and a man of conside-

rable eminence, asserted that fever was not so much a primary disease, as secondary and dependent on some more hidden disorder. In order to avoid the force of his doctrine, after physicians established the diversity of symptomatick and idiopathick fevers. Presently after Diocles, Erasistratus, a physician at the court of Antigonus, invited the attention of the world, his proximate cause resided in an error loci. Next Asclepiades, the Bythinian, stepped on the stage and rudely grasped the reins of government; he, adopting the doctrine of atoms, handed to the Greeks by Democritus of Abdera, attempted to account for the difference of types by a difference in the size of the corpuscles, which he supposed to be formed by a combination of indivisible atoms. Here emerges the doctrine of the obstruction of the permeable canals of the body, and its consequence modern viscosity and lentor, so famous in the schools. Asclepiades was the father and patron of the sect of the Methodicks.

Themison vibrated between *strictum et laxum*, and on those two pillars reared his pathology of diseases, here are the first traces of spasm, afterwards laboured by Hoffman and matured by the great Cullen: this hypothesis claimed the ascendancy, at Rome, for more than an hundred years. At last Galen, the impassioned admirer of Hippocrates, exhumed and reanimated the humoral errors.

Athenæus ventured to resuscitate the doctrine of the putrescency of the blood (this is to be found in the writings of earlier authors) and putrescency has not made a very despicable figure in the world. Avicenna expressly defines fevers to arise from a preturnatural heat of the heart.

The Galenists prevailed until the beginning of the sixteenth century ; about which time Aureolus Phillippus Theophrastus, commonly known by the name of Paracelsus, began to make a figure. This man assailed the Galenical party with all the engines of effrontery and resources of unimproved chymistry. Here commences the period of medical romance, so fraught with the struggles between the mechanical and chymical modes of reasoning, these eventually neutralized, and ushered in the chymico-mechanical philosophy.

The furious archeus of Van Helmont, differently modified, is the efforts of nature, so celebrated by Campanella and Sydenham, and autocrateia of Stahl. If we except Mundy, Borelli and Cole are the only writers previous to the time of Hoffman, who considered the nervous system as directly affording a seat for the proximate cause of fevers : Here in more positive terms is expressed the doctrine of spasmodick stricture. This idea of Hoffman, Cullen has elaborated to its utmost perfection. Who has taken the proximate cause from the heart, and

fixed it on the superficies of the body, in an atony and spasm of the capillaries.

After the above recital the mind is restless and in eager expectation waits to embrace a knowledge of the true nature and seat of the proximate cause. I shall, leaving the anterior whims to slumber with their authors, they being, by one obliterating stroke of the pen of Dr. Rush, sentenced to perpetual silence, take the liberty of putting the opinion of the celebrated Philadelphia Professor into the crucible of analytical inquiry. The opinion of Doctor Rush is the latest that I have met with in the writings of physicians. His words are "having premised these general propositions, I go on to remark, that a fever (when not misplaced) consists in a morbid excitement and irregular action in the blood-vessels, more especially in the arteries." "This irregular action is in other words, a convulsion in the sanguiferous, but more obviously, in the arterial system." Page 134, Vol. 4. "From the facts and analogies which have been mentioned, I have been led to believe that irregular action or a convulsion in the blood-vessels, is the proximate cause of fever." Page 139, Vol. 4.

There incontrovertibly is a difference between a fever and the proximate cause of a fever; a fever cannot consist in, or be made up of (these are synonymous) a convulsion in the blood-ves-

sels, and a convulsion in the blood-vessels be the proximate cause of a fever. An effect and the cause of that effect cannot be the same. If the fever consists in, or is made up of, an irregular action or convulsion of the blood-vessel, what is the proximate cause? and vice versa. It is illogical to identify cause and effect.

The proximate cause, of which writers say *quae presens facit, sublata tollit, mutata mutat*, after being hunted from one part of the body to another, and metamorphosed from one thing into another, at last takes refuge in the ignorance of its friends; having for its associates Phlogiston and the four elements; and an honourable triumvirate they form, each having enjoyed its apotheosis.

In a disease there are three essential causes. The predisposing, or an aptitude of the body to be acted on; the occasional, which acts directly or indirectly on the seat of life and action, and the exciting, or that which destroys the equilibrium of the nervous energy, and by this destruction of the equipoise of the system gives the occasional cause an opportunity of making an impression. The causation or *modus operandi* which takes place between the occasional cause and the living principle is not accessible even to the most vigorous efforts of the human mind.*

* Notwithstanding the generous and enlightened suggestions of the able and ingenious editor of the New-York Repository,

An offending entity (the remote or occasional cause) assails the tranquility of the healthy body, a particular, though inscrutable, infraction of the harmony of the animal œconomy is caused, evidencing itself by a chain of symptoms more or less unequivocal; these constitute the symptomata of writers, that (the primary disarray or infraction) the disease; here we observe a regular and simple concatenation of cause and effect, and the evidences of such an effect. We cannot apply the name of disease to an arrangement of symptoms, no more than we can the appellation of matter to an assemblage of qualities, or the epithet of spirit to a combination of modes,

the prepossessions of the author against the *possibility* of a proximate cause in any disease are still inveterate and incurable. So sceptical, indeed, is he on this head, that he apprehends there is no one thing more grossly absurd or completely unintelligible than the conception of agency or operation between the action of the remote cause on the living principle and the result, the disease, farther than the *modus operandi* or rule of action. And in his estimation it argues no great soundness of intellection to magnify a rule of action into a distinct agency.

All that has fallen in his way is so disgustingly puerile that he would feel relieved and gratified on seeing any thing assuming the form of rationality on the subject.

In his objections to the reality of a proximate cause the author is strongly fortified by the following sentence from the very learned Dr. Gregory.

“Haec quum ita sint, non mirum esse poterit multa medicorum commenta de causis *proximis* variorum morborum prorsus *futilia* esse, et non modo *falsa*, sed adio *confusa et obscura* ut vix possint intelligi, neque fortasse ab ipsis eorum auctoribus unquam *satis intellecta fuerint*.” Consp. Tom. 1, page 29.

but to that particularly morbid state of the body giving origin to such an arrangement of symptoms.

Chills, fever, pain, prostration of strength, discolouration of the tongue, &c are symptomatick of a hidden and essential disarray (a disease) of the nervous power. Figure, divisibility, extension, and solidity are indicative of an inscrutable material substratum. Passion, memory, and judgment are proofs of an immaterial essence, the nature of which the labour of the human mind cannot develop.

The cherishing beams of philosophy have begun to dawn, and I hope soon will enable us to proceed with more certain step in the healing art. It is time that we should divorce from our minds the *petitio principii*, and, like Pyrrho, disrobe ourselves of credulous facility. This epoch demands self-evident premises or proven data for the ground-work of our inductions. *Jurare in verba magistri*, is the motto of unthinking hebetude; a master's nod ought not to block up the avenues of research.

CHAPTER III.

DIAGNOSIS.

THAT assemblage of symptoms, which generally are the appendages of any disease, and establish a barrier between it and all others, constitute its pathognomonicks or diagnosticks. The general characteristicks, which disjoin the yellow fever from all others, are the following :

In most instances, a prostration of spirits and an inaptitude to motion, a sense of uneasiness and great fatigue ; pain and uneasiness through the limbs, as if from riding. It some times, without any premonition, impugns the guardians of life. It will in one instance assume the dress of the tertian, and in others clothe itself in all the characters of a cold. But let what will be its harbingers, it soon hangs out its own colours, and demands a tribute. The eyes become more or less affected by inflammation, accompanied with an acrimonious or burning epiphora ; the head feels itself molested by pain and giddiness, and a sense of congestion ; the tongue is indifferently white, yellow, blue, red, brown or black ; in the first days of the disease it has an oily feel. A pyrexia attends ; the skin is one while hot and dry, at other times preter-

naturally cold and clammy. The præcordia is much oppressed, attended by a great inclination to vomit. Vomiting not unfrequently, or a cholera morbus or a diarrhœa gives notice of the approaching calamity. The stomach in the latter stages of the disease labours under a sensation of having in it something which it cannot digest; this sensation they attribute to whatever they have swallowed: A flatulency and hiccough help to fill up the train of evils. There is pretty uniformly a paucity of urine, and what is voided is very high coloured. A black vomiting or purging, hemorrhages from every part of the body, especially the stomach, uterus, bowels, nostrils, and the incisions made by the lancet in bleeding; carbuncles and numerous little biles, more or less, act their part in this tragical scene; the black matter and hemorrhages seldom appear until after the fourth or fifth day; yet they sometimes occur earlier. The countenance has a peculiarly ferocious look. The eyes are with the redness, tinged with a croceous colour; this yellowness frequently diffuses itself through the whole superficies of the body.

There is in some cases, about the fifth or sixth day, a cessation of the fever, and all the violent symptoms, every thing becomes apparently favourable, and the physician will augur auspiciously; but this is a mournful circumstance; it is the powers of life ceding, and not a relaxation

of the disease ; especially if yellowness and hemorrhages co-exist. There is frequently a metastasis to the testicles.

In puking, the patient some times throws up nothing but what is taken into the stomach rendered a little ropy, at other times a black liquid, resembling a mixture of soot and water, is ejected.

The blood when abstracted is seldom covered with a buffy coat, but generally is what we call, a dense, red blood ; it rarely is dissolved. An obmutescence, and deafness are among the last marks of an approaching dissolution, they are truly prophetick of death. The sensibility of the surface of the body is exceedingly morbid ; and on the least touch communicates uneasiness ; this preternatural excitability I have met with in a surprising degree.

Not unusually a considerable degree of delirium accompanies this most prominent in the black catalogue of human ills. Perhaps there is no complaint, from the effect of which patients are so long convalescing. Small purple spots very often variegate the arms, breast, and neck ; they are ominous of peril.

By the dissection of defunct bodies, we get a view of the dreadful ravages of this relentless malady: We behold the stomach disfigured with sphacelated spots, and characters of inflamma-

tion. The liver swollen, and exhibiting every mark of phlegmasia: The spleen preternaturally flaccid: The gall-bladder turgid with black and acrid bile; the whole of the *primæ viæ*, when a natural diarrhœa co-operates, is manifestly affected with erysipelatous inflammation, which, by the way, is the species of inflammation that attacks the stomach, and this, probably, is the reason why the blood, when abstracted, is not sizzly; were it of the phlegmonoid species the size on the blood would uniformly appear; in some cases this species of inflammation does attend, and in those the pleuritick coat, pretty uniformly, proclaims its presence. The erysipelatous species is generally too rapid in its progress to mortification to communicate the buff to the general mass of blood. That the buffy coat is an inseparable and infallible sign of inflammation when accompanied by fever, and vice versa: See the experience of the most enlightened and assiduous practitioners; also the professors of Edinburgh; under whose wings, were folly and ignorance to deluge creation, learning and science would find shelter.

The lungs not unfrequently show marks of inflammation. The encephalon is pretty uniformly implicated in the testimony of the general destruction occasioned by the yellow fever operating on the animal frame: Its meninges

are found inflamed, the cortical and medullary substance is unusually red.*

This disease attacks sometimes with such violence and severity, as, either from its force or the feebleness of the patients, to supercede, by death, most of the above-mentioned appearances.

The black vomiting, hemorrhages, cerebral affections, yellowness of the eyes and skin, purple spots, and pyrexia being the most conspicuous and inseparable diagnosticks of the complaint, merit more seriously our particular discussion.

The bile, in its natural and healthy state, is as bland and mild as any secretion in the human body, but when the liver is affected by any specifick action, its secretory function, like similar physiological processes, is subject to vitiation ; and that this black matter, discharged indifferently upwards or downwards, is vitiated bile, depending on a morbid action in the secretory organ, is obvious. 1. From the great quantity which is found upon dissection, in the gall-bladder. 2. From its great acritude.* 3. From its analogy to other disordered secretions : For instance that of the kidney in diabetes, of the stomach in dyspepsy, of a sore when it becomes,

* Vide Pouppé Desportes, Hillary, Mosely, Rush, &c. &c.

as we generally phrase it, vitiated. Laudable pus is a secretion, so is the acrimony escaping from a vitiated, a cancerous, or a scrofulous ulcer. the atrabilis of the ancients perfectly accords with the black vomiting of the moderns. Some physicians have persuaded themselves that this black matter is owing to an admixture of blood, and that there is an absolute want of secretion in the yellow fever; this opinion, I must believe, has a more intimate affinity with prejudice, than with reflection.

Hemorrhages seldom overtake the patient in the early stage of the disease, except under the form of epistaxis, which is not commonly considerable. When they do occur, they are the indubitable evidence of an atony or paralysis of the blood-vessels and this atony in its turn depends on a destroyed vigour of the nervous power; this destruction of the nervous influence is effectuated by the peccant agency operating directly and immediately on the sensorium commune. It is the result of a violent action on the immediate seat of life, and not of the vascular, or arterial system; the blood-vessels can only be acted on secondarily. If we could timously diminish the quantity of the hostile power, we should infallibly prevent those sanguineous fluxes. Moderate blood-letting, by lessening the volume of the blood, will also contribute considerably to that end; but too profuse bleedings, by robbing the system of its sources of recovery

from so violent a shock, precipitate the unhappy sufferer to his grave. That a violent action on, and not a smothered or incontrollable exertion of the system, lays the foundation of those hemorrhages, is obvious. 1. As they do not accompany phrenitis or pneumonia where the vascular exertion is much more fierce, active, and oppressed. 2. Blood-letting after the second or third time, except in particular habits, when it is pretty copious, so far from relieving the imaginarily oppressed pulse, rather renders it more feeble and yielding. 3. In the most violent attacks, where there exists the greatest degree of indirect debility, the propriety and safety of immoderate phlebotomy are in the inverse ratio of this indirect debility. I speak from my own experience, and that of Hillary, and Pouppé Desportes who carried the lancet to its greatest extremes, and possessed the most ample opportunities of seeing its advantages and disadvantages ; not for one season only but for fifteen or twenty years, during which time he practised in the West-Indies ; as also of several of the most respectable physicians in the city of Baltimore. Lastly, hemorrhages are more apt to occur in those who have been copiously, than in those who have been moderately bled ; provided untimely death, superinduced by those large evacuations, does not obviate them. So is mortification about the orifices made by the lancet. Pouppé Desportes, speaking of those hemorrhages, and mortifications, remarks ;

“ Dans plusieurs les saignées se rouvrent, et le sang, malgré le nombre des compresses, pénètre, cette hemorrhagie est souvent accompagnée d'une gangrene charbonnée, qui se forme autour de la saignée, et dont on ne peut, arrêter le progrès.” And a little below.—“ Cet accident arrive ordinairement à ceux qui ont été trop saignés.”* I shall speak more at large on this when I arrive at the therapeutick division of this publication.

The Encephalon although subject to inflammation and partial infarction, cannot labour under general congestion ; that is, the brain-case cannot contain more at one time than another ; except the bones and sutures become firm and ossified, and in cases where the sutures are afterwards destroyed by diseases. The medullary substance of the brain is incompressible,† and the case itself is unyielding : When a congestion of the right side of the head happens, there is a simultaneous, and equivalent diminution of the *aræ* of the vessels of the left. In case of hydrocephalus there is a general invasion of the *aræ* of the cerebral vessels. In some instances the cortical substance is worn away by attrition. If a general congestion of the brain could take place, a partial vacuum would of consequence be possible ; and if a partial vacuum took place,

* Vide Tom. 1, page 200.

† Vide professor Munro's incontrovertible experiments.

the plates which are behind the eye-balls would unavoidably be forced in, and instant death be the result by the weight of the external air: The columns of which, in weight amounts to about forty-pounds on each eye, allowing fourteen pounds to each square inch.* The posterior plate that supports the eye is frequently so thin as to be quite diaphanous. The inference is that either an inflammatory or partially congested state of the brain must give support to that delirium, vigilance, &c. we observe in the yellow fever and many other complaints.

This recieves accession of certitude from the circumstances attending the decapitation of an animal; when the head of an animal is severed from its body, all the blood discharged is from the external parts of the head, not one drop escapes from the internal. This is substantiated by the following experiments. Dissect the vessels to their exit from the skull, then secure them firmly by a ligature, this being done, divide the case and you will find every vessel regularly filled and replete with its contents having suffered no evacuation.

Again, take a glass-globe with two opposite orifices, fill it with water, then put your finger upon one, and turn the other downwards; no water will escape until you remove your finger

* Vide Hale's experiments.

from the superior orifice. Nor can the contents of the skull be either decreased or augmented, except the case previously be exposed to violence.

The yellow or brown colour of the eyes and skin is owing to an absorption of the bile or brown matter after it is secreted. There is sometimes a temporary yellowness of the skin, this may be produced by a peculiar action of the blood-vessels.*

The purple spots constitute a pathognomonic, they are neither the production of an over action of the system, nor a dissolved state of the blood. They are to the yellow fever what the red spots are to the cynanche maligna, or the eruptions of the skin to the measles; they are symptomatick, and no regular consequence of the general state of the fluids or condition of the solids.

Fever is a convulsive action of the arterial system, as mentioned by Dr. Rush, accompanied by more or less of a peculiar and indistinguishable heat and dryness of the skin: The dryness and heat though are not always present; as in the febris typhodes. This convulsion of the arterial system is the result of a more inscrutable and hidden morbidness of the source and seat of life and action, the nervous power. The mus-

* Vide Rush.

cular fibre possesses no vis insita, its sensibility and contractibility are feudatory of the vis nervia.*

The source of life and action must be originally and primarily concerned in all the operations, whether morbid or healthy, of the animal body. The nerves are the "seat and throne" of all diseases.

Whether a fever be the result of a mere mechanical action, or the provident effort of the vis medicatrix naturæ, that power in the body to heal its own maladies, I leave to the more learned to determine. But there is something about a fever which is more easily recognized than developed.

A peccant entity, acting either mediately as in the form of fracture or wound, produces fever indirectly ; or immediately as in the condition of human or marsh effluvia, &c. produces fever directly. In both cases the fever is the consequence, and symptomatick : In neither is it strictly idiopathick. It is nothing but a symptom of a more primary and essential disturbance of the nervous energy.—This primary and essential disturbance is uniformly the disease ; whatever succeeds is only indicative of this first disturbance of the quiet and health of the animal system.

* Vide Monro's nervous system.

CHAPTER IV.

CURE.

WE now arrive at the division of this essay which is the most interesting and merits the most serious and unbiased investigation. And leaving the slippery declivity of hypothetical incertitude, we introduce our readers to the more unequivocal and inflexible data of practical experience : where feeble theory is supplanted by more certain practice, where the sick bed triumphs over the reveries of the closet.

Dr. Rush, in manfully and successfully labouring to stem the torrent of error and preposterous madness, which had diffused themselves throughout, and woven the tissue of general practice ; and in calling the medical mind back to the almost antiquated system of depletion so fortunately pursued by Sydenham, Cullen, Monro, Gregory, Botallus, Pouppe Desportes &c. has attached immortal honour to himself, and, using a gallicism, deserved well of mankind. The doctor's exalted dignity elevated him above the mean wiles of plagiarism. He, with the candour proper to great minds, frankly acknowledges his obligations to preceding writers. I am, however, considerably persuaded that in his strenuous exertion to crush the growing folly of medical pre-

judice, which took root in the execrable writings of Brown of Edinburgh, and Kirkland of England, he has suffered himself to be hurried within the embraces of the opposite extreme. It is beyond contradiction that the experience of some of the most scientifick and best informed physicians, will not warrant the extremes of depletion inculcated in his learned works.

In the management of the autumnal remittent or yellow fever there are four therapeutick intentions.

1. To diminish the violent action of the general system and remove as far as possible the inflammatory disposition of the liver, stomach, &c.
2. To take off the stricture of the superficies of the body.
3. To discharge the acrid bile as quickly as it is excreted.
4. To restore the vigour of the frame as soon as possible after the fever has subsided.

The first intention is best accomplished by a judicious and proper use of the lancet, together with a speedy introduction of mercury, either in the form of calomel or ointment, into the system. It will too often happen from an over excitability of the stomach that the calomel cannot be used. When this occurs the mercurial ointment must be freely applied to the insides of the

thighs, legs, and arms ; those parts being the most abundantly, of all the external parts of the body, supplied with the lymphatics.

Calomel is the most efficacious and powerful of all medicines in the resolution of inflammations of whatever kind they may be. But in those of the liver its salutary effects are peculiarly deserving our notice. For this valuable information we are principally indebted to Dr. Gilchrist of Scotland and the practitioners of the East-Indies.

When we enter the room of a patient in this fever our first attention should be to the state of the eyes, the degree of pain in the head, oppression about the præcordia, and the fullness of the pulse. This is seldom or never hard ; indeed the stroke of the artery is more deceitful in this fever, than in any other disease I have ever met with. If the eyes be much inflamed, or labour under a sense of protrusion from the sockets, or an unweildiness in their motion ; or an acrid epiphora frequently distils from the eyes ; together with the above if the head complains much, the pulse appear full to the application of the fingers, and be frequent, we must instantly have recourse to the lancet. Even should the tongue at the same time be blue or brown. The colour of the tongue is not to be in general confided in. It is sometimes blue or brown from the first days of attack. So long as the pain of the head, or back continues

with a very frequent or full pulse the lancet must be resorted to. This however will rarely be the case after two or three good bleedings, except in particular habits. Some habits will bear the lancet to the fifth or sixth repetition, especially where the indirect debility of the system is not great and where there is great heat of the surface of the body, and the tongue white.

A timous use of the lancet more effectually than any other remedy, tends to prevent the rapid progress of the inflammation of the different viscera. When it has taken place blood-letting promises more liberally than all other remedies. But when bleeding is carried to too great extremes, it exhausts the general system and prostrates the powers of life in so great a degree, that the animal frame can never renew its functions ; it hastens, by robbing the blood-vessels of internal support and nourishment, that atony and palsy of the vascular system which lays the ground-work of those melancholy hemorrhages.

When blood-letting is had recourse to, it should be practised within the first three or four days. It may, under particular circumstances, be performed at later periods, but not with such propitious consequences. I have practised it, as late as the tenth and fifteenth day of the disease, in instances where the patient's strength had not been sapped by evacuations.

Experience, together with the writings of most of the respectable practitioners of the West-Indies, establish the following.

Those who were not bled suffered from the neglect of the lancet,

Those who were bled largely died from the abuse of the lancet.

Those who were bled according to the pain of the head, fullness of the pulse, and oppression about the præcordia, or in other words moderately, in a much greater proportion recovered. Bleeding is by no means a new remedy in the yellow fever.

There were two young gentlemen, who in a visit to the Point contracted the disease : They were both taken ill a few days after their return to the Town. One of them was blooded six or seven times, and died. The other was bled once only and recovered. The violence of the attacks was apparently equal.

A little after, there were two others who took the disease by going to the contaminated atmosphere ; one of whom was bled six or seven times within about forty-eight hours, was put into the cold bath, had injections of cold water ; he died, amidst the hands of the operators, during the third injection of cold water. The other who was delirious almost from the first onset of the

complaint, was not bled at all, yet after a severe and dangerous illness recovered.

There were two other cases, one of whom lost about as much as would be taken away at four common bleedings, and recovered. Though no patient could be worse than she was to recover. The other was bled twelve or thirteen times, his arm mortified, and he departed from among the living. Out of six who took the disease at or near Bowley's wharf, five, some of whom were so much evacuated that their friends threatened to interfere, died. The one, Mr. Waters, who escaped was bled but twice and that moderately. There were some instances of recovery after those profuse evacuations, but they were relatively few. Where one, after such immense losses of blood escaped, there were ten who were either not bled at all, or but once or twice, that recovered. Out of all the blacks, for negroes by no means escaped this complaint, whom I attended from the first attack of the disease, and they were not few in number I lost none. I bled but one, and him only to the amount of six ounces. I do not recollect that I met with a single instance of hemorrhage in a black person.

Calomel not only is the most effectual medicine that can be used in the first stage, but is also the only one in which we can have confidence to remove the stricture of the surface of the body ; especially when assisted by the warm bath,

either generally or partially applied. I have seen the ephidrosis produced to that extent by a liberal use of calomel, as to require three or four changes of linen in twenty four hours.

Calomel not only cures by acting as a diaphoretick and antiphlogistick, but also by establishing in the system an opposite action to that of the fever. No two general actions can exist at the same time : So that when the mercurial action takes place the morbid one must, of necessity, cease. The establishment of this mercurial action is confirmed to the practitioner by a free ptyalism. Whenever a free salivation takes place the patient is safe. Perhaps no person ever died after the full establishment of this discharge from the gums. Not that the salivation, strictly speaking, is of any service in itself. It is in the yellow fever, as in the lues venerea, the unavoidable consequence of the general mercurial action of the system, and of no farther service than informing the physician of this general action.

That the local pain is of no advantage is evident from the following. Let the gums become ever so much inflamed, pained, and swollen, if a very free spitting should not succeed, the sick reaps no advantage, but, on the contrary this state of the gums is ominous of approaching death.

To invite the mercury to the surface of the body, the tepid bath should be used, or in its place the pediluvium, and local applications of flannel, wrung out of hot water, to the regions of the liver, stomach, &c. Those should be frequently repeated, and continued, at least, half an hour each time.

The cold bath has been very strenuously recommended by Jackson. But it does not appear from his writings that he ever used it with success without its being preceded by the warm bath : This together with its ill effects whenever it has been introduced into practice in America,* render it probable that Jackson attributed to the cold, what belonged to the warm bath. If it has ever been of the slightest utility in cases in America, these cases have not as yet found their way to the public. I say from the cold bath, in the practice of Dr Jackson, being uniformly preceded by the warm, and its general failure in America when used alone, I am fully persuaded that it not only is a useless, but a dangerous application in the yellow fever.

Emeticks even of the gentlest kind are inadmissible in every stage of the fever.† No preparation of antimony, from the tendency in those

* Vide Rush.

† Some practitioners have given emeticks in the instant of attack with supposed advantage.

articles to excite vomiting, can be used with safety. The justly celebrated James' powder is here of too precarious operation. All neutral salts are too inert and uncertain.

Whether, upon being called to the patient, we bleed, or not ; we must instantly order a mercurial purge, which should be repeated every day or every second day, so as to produce four or five stools daily, this number he at least ought to have. To assist the operation of the catharticks ; the lower part of the intestines must be opened by means of glysters. A glyster-syringe will with great propriety and utility be kept constantly in the patient's room, that by the use of this the purges when they are too slow may be quickened and invited downwards. For as above mentioned the sick should never have less than four or five passages a day during the strength of the fever. Catharticks are of all remedies the best and most useful. They are not to be omitted even in cases where the pulse is feeble, and intermitting : If purges cannot relieve the patient, his chance is truly melancholy.

In support of the great necessity of constant purging consult Moseley, Hillary, Pouppé Desports, and the learned Rush. I had my patients to call for the close-chair from three to ten and fifteen times a day. And those who purged most, when not accompanied by puking, recovered soonest. In one case which I was called to, four

grains of calomel produced thirty-five stools : after which evacuation the patient began to mend and recovered. A patient, to whom I was called, took a little broth which rendered him much worse than he had been the preceding day. I ordered him thirty grains of jallap and ten of calomel (my usual dose) which operated twelve times ; and from that instant he began to recover. It was to the repeated use of catharticks, and mercurial diaphoreticks that I trusted the blacks to whom I was called.

Of all catharticks, I prefer gamboge and calomel, or jallap and calomel or senna in powder and calomel. By the timely use of the former of these I prevented the regular course of this fever, in at least thirty patients, who upon the first appearance of the disease called on me.

The fourth and last intention is employed principally in selecting those articles which will the most readily restore the exhausted strength of the patient. From habit and prejudice, in this stage of the disease, physicians generally fly to well known powers of bark and wine, opium and æther, colombo, and quassia, &c. But although these medicines are in appearance our hope and reliance ; a just pathology and experience will quickly evince the impropriety and hurtful tendency of all tonick and stimulant remedies. The snake-root itself is too great a bitter, and must not, except under particular circum-

stances, be exhibited. Our sole trust and dernier resource is in a well regulated dietetick plan, with now and then a purge to keep the bowels open. Nor must the inexperienced suffer the weakness of the patient to frighten them from the free use of purgatives.

The habits & desires of the stomach are chiefly to be consulted after the fever has subsided. But during the height of the fever, neither flesh nor any thing made of flesh can be allowed. Vegetables are the only articles of food that the patient can be indulged in while the fever possesses any considerable strength.

The patient must never take any beverage stronger than barley-water, lemon-ade, cold water, water with a toast in it, and such like mild potables. Some physicians are too cautious in giving cold water, and lemonade during the use of mercury. But I am satisfied from daily practice that no drink is more innocent and beneficial in a fever, especially the yellow fever, than lemonade, and cold water : nor are we to discontinue the citrick acid even during the use of calomel. Chymical affinities might induce us to believe that upon an union of the acid and calomel, the latter would, by decomposing the former, and attaching to itself part of oxygen, convert itself into the oxigenated muriate of mercury, or what is vulgarly called corrosive sublimate. This, however in fact does not happen.

If any acid possessing a greater affinity, to the base of calomel, than the muriatick, be used, the muriatick acid will be precipitated, and a new neutral, formed by this more powerful acid, and the base of the calomel viz. mercury, will be the consequence.

Blisters are sometimes useful ; particularly when applied to the epigastrick region. They quiet the disturbance and excitability of the stomach. They are not unusually applied to the extremities and other parts of the body but perhaps more from custom and fashion than conviction of their real use.

Opium may, in the last stages when there is no fever present, be given in very small quantities, thereby to take off for a few hours the great irritability of the stomach. But all opiates must be followed, within six hours after they are given, by catharticks ; other wise their stimulant qualities will far more than counterbalance any advantages the patient may at first receive from them.

During the dying state of a patient, I have made use of æther and musk, but my views contemplated the removal of particular symptoms, such as hiccough, twitches of the tendons &c. which very much distressed what few friends there were who had courage enough to come near the departing sufferer ; and by no means the cure of the complaint. They serve under

such circumstances to smooth the passage to the grave but they cannot deliver from the grasp of the fell malady.

When hemorrhages do overtake the unhappy sufferer, general experience has painfully convinced us of the impossibility of managing them by astringents whether internal or external, except in those of the uterus where applications of cold vinegar for the most part answer. We are here still to pursue our general plan.

All applications to the carbuncles which oft accompany this fever, are useless. They may do mischief but we can expect but little benefit from them. Nor does opening of them seem to answer any good end. It is best to leave them to nature.

The first part of the paper is devoted to a general
discussion of the problem. It is shown that the
problem is equivalent to the problem of finding
the minimum of a certain function. This function
is then shown to be convex, and the minimum
is found by the method of steepest descent.
The second part of the paper is devoted to a
detailed discussion of the method of steepest
descent. It is shown that the method is very
efficient for finding the minimum of a convex
function. The method is then applied to the
problem of finding the minimum of a certain
function. The results of the calculations are
given in the following table.

Iteration		Value of function	
1	0.0	1.0	0.0
2	0.1	0.9	0.1
3	0.2	0.8	0.2
4	0.3	0.7	0.3
5	0.4	0.6	0.4
6	0.5	0.5	0.5
7	0.6	0.4	0.6
8	0.7	0.3	0.7
9	0.8	0.2	0.8
10	0.9	0.1	0.9

The results of the calculations show that the
method of steepest descent is very efficient for
finding the minimum of a convex function. The
method is then applied to the problem of finding
the minimum of a certain function. The results
of the calculations are given in the following
table.

APPENDIX.

DURING the time that the foregoing Tractate was at the press I accidentally met with the answer of the Physicians of Philadelphia, to the request of the governor, the hon. Thomas Mifflin—in relation to the yellow fever. A perfect and harmonious coincidence unites our sentiments, so far as they respect the origin and cure. For the satisfaction and benefit of the public, I will insert the letter.

SIR,

“ IN compliance with your request, the subscribers have devoted themselves to the investigation of the origin, progress, and nature of the fever which lately prevailed in our city ; and we have now the honour of communicating to you the result of our enquiries and observations.

We conceive the fever which has lately prevailed in our city, commonly called the yellow fever, to be the bilious remitting fever of warm climates, excited to a higher degree of malignity by circumstances to be mentioned hereafter.

Our reasons for this opinion are as follows :

1st. The sameness of their origin, both being the offspring of putrefaction. Of this there are many proofs in the histories of the yellow fever in the West-Indies. Where there are no putrefaction the West-India islands enjoy a perfect exemption from that disease in common with northern climates.

2d. The yellow fever makes its appearance in these months chiefly in which the bilious fever prevails in our country and is uniformly checked and destroyed by the same causes ; viz. heavy rains and frosts.

3d. The symptoms of the bilious fever are the same in their nature. They differ only in their degree. It is no objection to this assertion that there is sometimes a deficiency or absence of bile in the yellow fever. This symptom is the effect only of a torpid state of the liver, produced by the greater force of the disease acting upon that part of the body. By means of depleting remedies this torpor is removed and the disease thereby made to assume its original and simple bilious character.

4th. The common bilious and yellow fever often run into each other. By depleting remedies the most malignant yellow fever may be changed into a common bilious fever and by tonic remedies improperly applied, the common bilious fever may be made to assume the symptoms of the malignant yellow fever.

*5th. The common bilious and yellow fevers are alike contagious; under certain circumstances of the weather and of predisposition in the body. That the common bilious fever is contagious we assert, from the observations of some of us, and from the authority of many Physicians who have long commanded the highest respect in medicine.

* This argument, the spirit of which states this and the bilious fever to be contagious, appears to be the offspring of conjecture and a biased education. Whatever the exertions and perspicacity of some individuals of Philadelphia may have explored I cannot determine. But, in my candid opinion, the observations, which deceived them into the persuasion that the bilious fever is infectious, are, somewhat defective. Many physicians of eminence have asserted, in their writings, the above fever to be contagious; but they have neglected to accompany those conjectures with tributary facts to support and give strength to them.

There is not, so far as my reading extends, one solitary case of a contagious bilious fever in the records of medicine: I say not one case wherein contagion has been, or can be proved. And here, reader, my opinion is propugned and countenanced by Cullen, Sydenham and many more of the most learned and experienced of physicians. Bilious fevers spread by what those learned gentlemen in the latter part of the following paragraph

6th, The yellow and mild bilious fever mutually propagate each other. We conceive a belief in the unity of these two states of fever to be deeply interesting to humanity, inasmuch as it may lead patients to an early application for medical aid, and physicians to the use of the same remedies for each of them, varying those remedies only according to the force of the disorder. It is no objection to this opinion that, that state of bilious fever called the yellow fever, is a *modern* appearance in our country. From certain revolutions in the atmosphere as yet observed only but not accounted for by Physicians, diseases have in all ages and countries alternately risen and fallen in their force and danger. At present a constitution of the atmosphere prevails in the United States which disposes to a fever of a highly inflammatory character. It began in the year 1793. Its duration in other countries has been from one to fifty years. It is not peculiar to the common bilious fever to have put on more inflammatory symptoms than in former years. There is scarcely a disease which has not been affected in a similar way by the late change in our atmosphere and that does not call for a greater force of depleting remedies than were required to cure them before the year 1793.

7th. And lastly. The yellow fever affects the system more than once, in common with the bilious fever. Of this there were many instances during the prevalence of our late epidemick.

The fever which lately prevailed our city appears from the documents which accompany this letter to have been derived from the following sources:

call "a constitution of the atmosphere which prevails in the United States and disposes to a fever (the bilious or yellow fever) of a highly inflammatory character. It is this constitution, effectuated by the marsh effluvia or vegetable putrefaction being diffused through the air, which in the first place gives rise to, and afterwards renders epidemick or general the bilious or yellow fever, as those gentlemen in what follows of their reply wisely allow. Those who wish to be satisfied that this fever is not contagious, will do well to pay particular attention to the cases instanced in the above letter.

1st. Putrid exhalations from the gutters, streets, ponds and marshy grounds in the neighbourhood of the city. From some one of these sources we derive a case attended by Dr. Caldwell on the 9th of June—one attended by Dr. Pascalis on the 22d of July, and by two cases attended by Dr. Rush and Dr. Physick on the 5th and 15th of the same month ; and also most of those cases of yellow fever, which appeared in the northern parts of the city, and near Kensington bridge, in the months of August, September and October. We are the more satisfied of the truth of this source of the fever from the numerous accounts we have received of the prevalence of the same fever and from the same causes, during the late autumn in New-York and various parts of New-Jersey, Pennsylvania, Maryland, Virginia and South Carolina, not only in sea ports but inland towns. The peculiar disposition of these exhalations to produce disease and death was evinced early in the season by the mortality which prevailed among the cats, and during every part of the season by the mortality which prevailed in many parts of our country among horses. The disease which proved so fatal to the latter animals is known among the farmers by the name of the yellow water. We conceive it to be a modification of the yellow fever.

2ndly. A second source of our late fever appears to have been derived from the noxious air emitted from the hold of the now Navigation, capt. Linstroom, which arrived with a healthy crew, from Marseilles on the 25th of July, and discharged her cargo at Latimer's wharf, after a passage of eighty days. We are led to ascribe the principal part of the disease which prevailed in the south end of the city to this noxious air, and that for the following reasons :

1st. The fever appeared first on board this vessel and in its neighbourhood, affecting a great number of persons nearly at the same time, and so remote from each other that it could not be propagated by contagion.

2dly. There was in the hold of this vessel a quantity of vegetable matters, such as prunes, almonds, olives, capers, and several other articles, some of which were in a state of putrefaction.

3dly. A most offensive smell was emitted from this vessel after she had discharged her cargo, which was perceived by persons several hundred feet from the wharf where she was moored.

4thly. A similar fever has been produced from similar causes, in a variety of instances : we shall briefly mention a few of them.

At Tortola a fever was produced in the month of June, in the year 1797, on board the ship *Britannia*, capt. James Welch, from the noxious air generated from a few bushels of potatoes, which destroyed the captain, mate, and most of the crew, in a few days.

Two sailors were affected with a malignant fever, on board the ———, capt. Thomas Egger, in the month of March, 1727, from the noxious air produced by wine that had putrified in the hold of the ship, one of whom died soon after her arrival in Philadelphia.

In the month of June, 1793, the yellow fever was generated by the noxious air of some rotten bags of pepper on board a French Indiaman, which was carried into the port of Bridgetown, by the British letter of marque *Pilgrim*. All the white men and most of the negroes employed in removing this pepper, perished with the yellow fever and the foul atmosphere affected the town, where it proved fatal to many of the inhabitants.

On board the *Busbridge* Indiaman a yellow fever was produced in the month of May, 1792, on her passage from England to Madras, which affected above two hundred of the crew. It was supposed to be derived from infection, but many circumstances concur to make it probable that it was derived from noxious air. The absence of smell in the air does not militate against this opinion, for there are many proofs of the most malignant fevers being brought on by airs which produced no impression on the sense of smelling. This is more frequently the case when the impure air has passed a considerable distance from its source and becomes diluted with the purer air of the atmosphere.

Several cases are related by Dr. Lind, in his treatise upon fever and infection of the yellow fever, originating at sea under circumstances which forbade the suspicion of infection, and which can only be ascribed to the impure air generated from putrid vegetables.

So well known, and so generally admitted is this source of yellow fever in warm climates, that Dr. Shannon, a late writer upon the means of preventing the diseases of warm climates, in enumerating its various causes expressly mentions "the putrid effluvia of a ship's hold."

We wish due attention to be paid to these facts, not only because they lead to the certain means of preventing one of the sources of this fever, but because they explain the reasons why sailors are so often its first victims, and why from this circumstance the origin of the disease has been so hastily, but erroneously ascribed solely to importation.

The fever which prevailed along the shore of the Delaware, in Kensington, and which proved fatal to Mr. Joseph Bowers and two of his family, we believed originated from the noxious air emitted from the hold of the ship *Huldah*, capt. Wm. Warner. This air was generated by the putrefaction of coffee which had remained there during her voyage from Philadelphia to Hamburgh, and back again.

* In the course of our enquires we were led to suspect one source of our late fever to be of foreign origin. The sails of the armed ship *Hinde*, on board of which several persons had

* Relatively to the persons affected after receiving the sails of the armed ship *Hinde*, there is not the smallest probability that they were infected by the sails acting as fomites. The explanation given by those gentlemen is perfectly satisfactory. And had not the snow Navigation been in the vicinity, we should not have been at a loss to account for their indisposition without admitting the idea of contagion. The sails emitting an offensive smell is an irrefragable proof that they were damp; and what must be the state of the water and deleterious gas which were enveloped in their folds? This putrid state of the water is the very postulate to render it destructive.

died of the yellow fever, on her passage from Port-au-Prince, and which arrived on the 4th of August, were sent to the said store of Mr. Moyse. Four persons belonging to the loft were soon afterwards affected with symptoms of a bilious yellow fever. We shall not decide positively upon the origin of the fever in these cases ; but the following facts render it probable that it was not derived from the persons who had died of it on board the suspected vessel. 1st, The sails emitted an offensive smell ; 2d, three of the cases of the persons affected in the sail loft were of a mild grade of the fever ; 3d, the fever was not propagated by contagion from any one of them ; 4th, the sail loft was within the influence of the noxious air which was emitted from the hold of the snow Navigation, being not more than fifty yards, and was in the direction of the wind which blew at that time over her. The extent of this air has not been accurately ascertained, but many analogies give us reason to believe that it may be conveyed by the wind, in its deleterious state, from half a mile to a mile.

In support of the opinion we have delivered of the origin of our late fever, we must add further, that in that part of the city which lies between Walnut and Vine-streets, and which appeared to be free from the effects of exhalation and the noxious air of the ships, there were but few cases of the fever which appeared to spread by contagion, even under the most favourable circumstances for that purpose.

Having pointed out the nature and origin of our late fever, we hope we shall be excused in mentioning the means of preventing it in future. ———— These are,

It is impossible that Philadelphia, amidst the numerous sources of vegetable exhalations could have one street or lane free of this miasma. And the dispersed cases which happened between Walnut and Pine-streets were produced by a less quantity of these vegetable effluvia.

There is not in this, nor any other writings of the Physicians of Philadelphia, a single case accompanied by even the probability of contagion. And there is no axiom more evident than that the bilious or yellow fever is not contagious.

* First. A continuance of the present laws for preventing the importation of the disease from the West-Indies, and other parts of the world where it usually prevails.

Secondly. Removing all those matters from our streets, gutters, sellers, gardens, yards, stores, vaults, ponds, &c. which by putrifaction in warm weather afford the most frequent remote cause of the disease, in all countries. For this purpose we recommend the appointment of a certain number of physicians whose business it shall be to inspect all such places in the city, the northern liberties and Southwark, as contain any matters capable by putrefaction of producing the disease and to have them removed.

Thirdly. We earnestly recommend the frequent washing of all impure parts of the city in warm and dry weather, by means of the pumps, until the water of Scuyllkill can be made to wash all the city ; a measure which we conceive promises to our citizens the most durable exemption from bilious fevers of all kinds, of domestick origin.

Fourthly. To guard against the frequent source of yellow fever from the noxious air of the holds of ships, we recommend the unlading all ships cargoes liable to putrifaction at a distance from the city, during the months of June, July, August, September and October. To prevent the generation of noxious air in the ships, we conceive every vessel should be

* This disease can only be imported from the West-Indies or any place by vessels arriving from those places with putrid water or vegetables in their holds, &c. As this fever uniformly arises from the unsound vegetables or putrid water, and never from clothes as the vehicles of contagion, or bodies which may be diseased on board such vessels ; our chief attention, instead of being confined to the persons, should be directed to the state of the cargoes ; which by the by ought always, during the sickly months, to be examined before they are suffered to come so near a city as that the noxious air can injure the inhabitants.

I have not annexed this appendix solely for the purpose of adjusting the etiquette of science, but in order to preserve the excellent preventive observations contained in the reply to the request of the governor of Pennsylvania.

obliged by law to carry and use a ventilator, and we recommend in a particular manner the one lately contrived by Mr. Benjamin Wynkoop.—We believe this invention to be one of the most important and useful that has been made in modern times, and that it is calculated to prevent not only the decay of ships and cargoes, but a very frequent source of pestilential diseases of all kinds in commercial cities.

In thus deciding upon the nature and origin of our late fever we expect to administer consolation to our fellow citizens upon the cause of our late calamity, for in pointing out its origin to the senses, we are enabled immediately and certainly to prevent it. But while the only source of it is believed to be from abroad and while its entrance into our city is believed to be in ways so numerous and insidious as to elude the utmost possible vigilance of health officers, we are led in despair to consider the disease as removed beyond the prevention of human power or wisdom. It has been by adopting measures similar to those we have delivered for preventing pestilential diseases, that most of the cities of Europe, which are situated in warm latitudes, have become healthy in warm seasons and amidst the closest commercial intercourse with nations and islands constantly afflicted with those diseases. The extraordinary cleanliness of the Hollanders, was originally imposed upon them by the frequency of pestilential fevers in their cities. This habit of cleanliness has continued to characterize those people after the causes which produced it have probably ceased to be known.

In thus urging a regard to the domestick sources of the yellow fever, we are actuated by motives of magnitude, far beyond those which determine ordinary questions in science. Though we feel the strongest conviction that the value of property, the increase of commerce and general prosperity of our city, will be eminently forwarded by the adoption of the foregoing propositions, yet these are but little objects in our view when compared with the prevention of the immense mass of distress which never fails to accompany a mortal epidemick. We consider ourselves moreover as deciding upon a question, which is

to effect the lives and happiness not only of the present inhabitants of Philadelphia, but of millions yet unborn in every part of the globe.

We are with the greatest respect,

Sir,

Your very humble servants,

Benjamin Rush.
Charles Caldwell,
William Dewees,
John Redman Coxe,
Phillip Syng Physick,
James Reynolds,
Francis Bowes Sayre,
John C. Otto,
William Boys,
Samuel Cooper,
James Stuart,
Felix Wise,
Joseph Strong.

His Excellency }
 THOMAS MIFFLIN. }

From Dr. Davidge to Dr. Barton.

DEAR SIR,

I AM entering on a compartment to which I am no stranger. Its topography is familiar to me : I have traversed its fields ; passed through its walks ; and explored its most intimate recesses, with great industry, care, and minute observation. At least every day of my life, I am engaged in the contemplation of the objects of this divison, and ought, in some measure, to be acquainted with them.

To be qualified to judge accurately, and well, of what is without the scheme of nature, we should be well acquainted with what is within her plan. A partial view of her operations will lead us into error, and inconclusive deduction. When I cast my eyes over the vast scenes of nature, I with much delight observe her plan ; I see animals, vegetables, and minerals : animals and vegetables living, and reproducing ; minerals growing, each after its own kind.

When I look into the body of a human female, I behold organs of higher and lower importance : the lungs to oxidate the blood, and the stomach to digest the food, the kidneys to secrete the urine, the uterus to bear the young &c. The

uterus and the ovaria, with their appendages, are equally with the stomach and lungs, provided by nature. They are not morbid productions. Each organ by nature is destined to certain functions, or offices. Am I deceived in this ? Can the ovaria and uterus be organs of nature, and yet conception and gestation be “grades of disease ?” Can any human mind, in its calmer moments, when relieved from the importunate entreaties of a new-born theory, conceive any thing so incongruous, as that the ovaria and uterus are organs of nature, and yet conception and gestation are gradations of disease ? Let us examine into this opinion.

In every perfect female body, we find organs of generation, as well as organs of digestion and respiration. Our venereal appetencies are as regular as our appetites for aliments of life, or our demands for the renewal of the oxygenous stimulus to our blood ; and it is as natural to gratify the one as the other. If we cease to gratify our appetites for food, or demands for respiration, we die ; if we cease to indulge our appetites for venery, our kind becomes extinct. The former are death to the body ; the latter is death to the race.

Now, seeing that the organs of generation, with their appetencies, are strictly conformable to the original intentions of nature, with what modesty or apology do we say, that the result

of these appetencies, and the functions to which the organs are destined, are gradations of disease? Can an organ with its instinct be natural, and nevertheless the only function of which it is capable be morbid, or a gradation of disease? Whenever a function is performed agreeably to the fundamental and immutable laws of nature, and without which intermediate function the grand scheme of nature becomes broken and discontinuous, we intelligibly and with understanding argue such function to be natural, or within the plan of nature. Of the grand though inscrutable catenation of human reproduction, that pregnancy is a distinguished link, no man in the possession of a sound mind can entertain the most extenuated doubt. If, then, a link connecting and alone appending the subsequent to the precedent generation, how can it be said to be a disease? A disease is a mere contingency; a contingency declarative of an aberration from the healthy economy of the animal body. Disease is an accident to which nature is liable, but no part of her economy can it constitute. But conception and gestation are the very work of the maturity and health of the animal body.

Were pregnancy a disease, or, as gentlemen please to phrase it, a *grade* of disease, then were pregnancy, in its inception, progression, and termination, truly fortuitous and incidental. The sublime order of the universe would be forced from its connection, its great design be marred,

ruined, and a second chaos involve its beauties. Pregnancy is within the control of uniform, determinate laws, consecutively subject to the common government of the economy of the body. The uniformity and regularity of utero-gestation are inferrible from the fixedness and constancy of its term.

That pregnancy is laid out and planned, in the deep, unsearchable projection of nature, is deducible from the accordance of the marriage state, bearing children, with the most perfect health. Not unfrequently, indeed, the stimulus of matrimony bears the body above diseases, not otherwise manageable by our art. That women, while pregnant, should be favoured with a total exemption from disease, is not at all consequential of the position, that gestation is a condition of nature. Nature may as readily be encumbered by disease, its various ramifications entwining about her springs, in one part of her motions as another. Dyspepsia at times invades the stomach, but still digestion is a natural function; tubercles may be diffused through the lungs, interrupting their play, nevertheless respiration is a healthy animal operation.

Can a part of the economy of nature be so ameliorated, by medical aid, as to be better suited, in its relations and operations, to the purposes of its office, than it is by its original constitution? I answer in the negative. Then why shall we

break in upon nature's works with our agency? Gentlemen not only upturn the foundations of the physical world, but they dream of suspending the very denunciations of Heaven on the point of their lancet.

During gestation, nature may, in her economy, be encumbered by disease; and the plan of her procedure be disturbed. But, as the cause of this disturbance is not uniform in its specifick relations, neither can the means appealed to be without variety in their kind. To the particular character of the disease, with which the pregnant female may be oppressed, the remedies, in extent and nature, must be accommodated. If the powers of life fall low, and she is unnerved by languor, arising from luxury or poverty of diet, let her be advised to suitable nutriment, cordial beverage, exhilarating company, and regular exercise. If the stomach be distressed by dyspepsia, she is directed to magnesia, alkalies, emeticks, tonicks, or lime-water; to which we add exercise, the first and best mode of infusing vigour into the system, with all its organs. If the bowels be slow, aperients are pressed into service. If the body be raised in its action by too high stimulation, general or local, the circulation is to be tamed, the system tranquillized, the rigid fibre relaxed, by the abstraction of blood; which abstraction, in degree, will be according to the accidents of each individual case, the general hurry, or local impediment. All general

rule is inadmissible ; there can be no uniformity of usage, in things of themselves contingent.

As I advance, I find that I have some matters of etiquette in science to adjust with an ingenious gentleman of Wilmington, Dr. Vaughan, and also with a learned lecturer of Philadelphia. While I use towards these gentlemen every personal civility and courtesy, I must be permitted to indulge in liberal criticism on their sentiments. Error in youth is excusable, but in teachers should be chastised.

I proceed, in the first place, to notice, in detail, some propositions from the pen of Dr. Vaughan, a gentleman of much character and ingenuity, and who, by writing, has become a teacher in the art of Midwifery. This gentleman lays it down, as a part of his ground-work, that "several pounds of blood are retained with the mother, and transformed into fœtal organization, and that this fluid is the menses, reserved during ten lunar months."

Dr. Vaughan promised, that, "after giving Mr. White's opinion, with its authorities, in his own language, he would reply to them in detailed order." He then subjoins : "It is immaterial to the present question, whether the catamenia be occasioned by general plethora or not; if a given quantity of the sanguiferous fluid, ordinarily discharged by essential laws" (surely

not a morbid hemorrhage dependent on essential laws), “and retained in the pregnant state, the consequences are the same.” But if this given quantity be unequal to the sum of the child’s weight, the consequences will be very dissimilar: and that it is unequal to it, we infer from the respectable testimony of Dr. Vaughan himself.

In his foot-note, he says, that “the weight of the full-grown fœtus, and its appendages, so far exceeds the ordinary sum of the catamenia in ten lunar months, that other excretions must be lessened in a considerable degree.” Rep. v. 6, p. 152.

This foot-note gives to me all I contend for; it maintains, in the face of all opposition, that there cannot possibly be “a retention or accumulation,” in either the general or uterine system, during the latter months of pregnancy. And especially if there is this contribution laid on other excretions, what occasion can there be for the use of the lancet?

The Doctor then refers us in a summary way, to the opinions of Dr. Rush: opinions always respectable, and entitled to the highest deference from the medical world. But opinions and detailed arguments are not the same, in my view.

At this moment my pen is arrested by the melancholy tidings, that my able and worthy

correspondent has paid the great debt of mortality. In his mind burned the lamp of science ; from his heart rose the incense of piety ; and through his actions flowed the warm stream of benevolence. But, alas ! nature has her demands, and in a tone and style too forcible and intelligible to be misunderstood, speaks to her son : “ dust thou art, and unto dust shalt thou return.”

The menses are, with me, a natural secretion from the arteries of the womb under ovarious influence. They are the first to premonish even the tender virgin herself of nubile maturity. They inspire their lovely authoress with new desires, admonish her to new hopes, and throw about her air all the ornament and force of irresistible captivation, attraction, and grace.

When this conceptionous female, from sexual intercourse with her manly companion, becomes pregnant, the uterine arteries cease their functions ; a new, though temporary, organ begins its office. The placenta, endowed with the function of a gland, provides nutriment for the fœtus. There are solitary cases, in which a species of morbid hemorrhage, or vicarious menses, continues throughout pregnancy. Is this efflux of blood from the vessels of the os tinæ ?

But to return, and fairly examine into the merits of the proposition, giving it the fullest

possible bearing on the subject. I will, for the moment, concede the point before us. Let the menses be formed, and be retained, according to the pleasure of the writer. The total sum of fluid, amassed during nine calender, or even *ten lunar* months, would not be, upon the most liberal estimate for fœtal organization, more than five pounds of aliment, allowing six ounces to each menstruous period. An ordinary child weighs from ten to fifteen pounds : that is, from five to ten pounds more than the total sum of the fluid retained.

How far does this go to demonstrate, that fœtal organization derives its sources from the menstruous fluid? And here I might, not inaptly, subjoin a second problem: If the human fœtus be made up of the menstruous fluid, whence are the materials for the organization of the young of the lower animals furnished? They have no menses.

The Doctor's proposition extends much farther. It not only insinuates, but directly maintains, that this fluid is retained, and yet is transformed into fœtal organization! What! is the menstrual fluid retained with the mother, constituting a source on which we may advantageously draw with the lancet, and moreover is transformed into fœtal organization? This is a species of logick, to my understanding, neither forcible nor clear.

The doctrine is wholly wrong, both in its premises and conclusion; and, of all hypotheses within the reach of a sprightly fancy, the most unhappy in its deductions for the general rule of blood-letting, with which it was proposed to quadrate.

Still further to illustrate his favourite doctrine of blood-letting during utero-gestation, this gentleman refers us to the concentration of excitement in the gravid uterus, and the extra-vitality of impregnation; to the tenacity of pregnant females for life, and the surviving existence of the fœtus in utero, after the death of the mother. I am not prepared to determine, whether an impregnated female is more vivacious than an unimpregnated one. There are not sufficient documents before the public, for us to enter on this disquisition. That a fœtus in utero survives five minutes, or even two, the death of the mother, is what the facts hitherto furnished by faithful obstetrick record directly oppose. The child ceases to live, immediately after it ceases to receive, through the mediation of the placenta, oxigen from the mother's blood. The chick may have its blood oxidated through the shell with which it is encased, but the human fœtus cannot have this necessary supply through the walls of the abdomen.

Obstetrick history expands to our view many instances of judicious, well-managed efforts to

save the child on the death of the mother ; but their ill success has devolved the iteration of these efforts on those who can believe, that the fœtus can live without the constant renovation of the oxigenous stimulus.

We have no materials of which the theory of the continuity of circulation, between the mother and child, can be constructed. Injections, thinner than red-blood, have not, as yet, found their way from one to the other. And, were the circulation continuous, and immediate, a lifeless mother could not support a living child. The alimental supply before birth, as before observed, is by means of the placenta, acting as a gland, as it is subsequently furnished by the mammæ.

Dr. Dewees, in his thesis, the mirror from which all the features of his doctrine are reflected to us, adopts the hypothesis, " that they (pregnancy and parturition) ought to be considered as diseases, according to the opinion of Dr. Rush, one of the greatest ornaments of medicine, in the present or any antecedent age. This he infers from the necessity, in too many instances, a few cases only excepted, of our being obliged to mitigate *their* violence, or shorten *their* duration*."

* " Pregnancy," " Though a *natural* alteration of the animal economy, which every female seems originally formed to undergo, and hence *not* to be considered as a state of *disease*," &c.

To this I can affix no definite, determinate idea. "Shorten their duration!" And are we obliged, in the general, to shorten the duration of pregnancy? The Doctor cannot possibly be serious in this; yet he says, that, "we are *obliged*;" it is not a point of choice, "to mitigate *their* violence, and shorten *their* duration," viz., the *duration of pregnancy* and parturition. Then pregnancy has no fixed, legitimate term! It is as salutary and regular at six, as at nine months! Would the extraction of a given quantity of the blood of the chesnut-tree improve the maturation of its nut, or aid the evolution of the burr?

A rose, whether cultivated in America, or transplanted to the soil of Russia, howsoever altered in its foilage, its efflorescence, or the tints of its petals, continues to be one of the chief *natural* ornaments of the parterre. Pregnancy remains, amidst all the mutations of climate, and cultivation of civilization, a natural condition; an indefeasible right of nature.

"The uterus is a hollow viscus, in which the great *object* of conception is performed." To my understanding, this sentence is extremely obscure; it is wholly unintelligible. "The uterus is a hollow viscus, in which the great *object* of conception is performed." The author certainly does not wish to convey the idea, that the uterus is the organ of conception; and yet to me,

the sentence is unsusceptible of any other interpretation. It leaves no room for his brilliant discovery of the passage of the second ovum, in case of superfœtation, along the fallopian tube ; its separation of the membrana decidua from the parietes of the uterus, and getting within this hollow viscus. Of all the discoveries in the mystery of generation, this is by far the most splendid*. An embryo, with its water and involucra, the whole not equiponderant with two grains, forcibly breaking up the attachment of the membrana decidua, and making for itself room in the cavity of the uterus ! Does this embryo force its way by mechanical powers, or melt down the band of attachment between the lining membrane and wall of the uterus, by certain chemical properties ?

The sentence in itself is a handsome display of the ability with which some gentlemen are favoured, of making a pretty arrangement of words,

* " This resistance will, however, be soon overcome ; either by the ordinary efforts of the tube, or by the ovum resting unusually long, and beginning to develope, obliging the mouth of the tube to open," &c. *Museum*, V. I, No. 2, p. 172.

What efforts are these, that can overcome the resistance offered by the attachment of the lining membrane of the gravid uterus ? The more the ovum develops, the less the probability of a passage through the mouth of the tube. The ovum, under these circumstances, must remain in the tube, and constitute an extra-uterine fœtus. Perhaps the ovum would travel into the uterus, as Dr. Harrison's semen would travel out of it.

See Museum, V. I, No. 1, p. 39.

without infusing into them the power of making a definite impression on the reader's mind.

But again to the thesis. "However easy the act of child-bearing be, among savage tribes, and certain individuals in various states of society, we find it, among others, an operation of great pain, and frequent danger."

Here is an assumption of principles, upon the begged question; a gratuitous assumption of the very points at issue. The sentence, with the following parts of the paragraph, of which it is a member, embraces as acknowledged facts, two points: the one, that the parturient act with the savage is uniformly easy, without pain or difficulty; the other, that, with the civilized woman, civilization and refinement have produced difficulty, pain and danger. Neither of these positions are true.

The first rests upon the insufficient grounds furnished by the reports, vague and highly questionable, of travellers. Travellers are privileged men; in an especial manner so, when they undertake to write and speak of things, to which they could not possibly have access. Whatever is uncommon, or without the usual course of things, is, by a savage, uncultivated people, in a high degree deserving of notice; is among the first objects of communication to a stranger. The reports, in themselves, convey to me the

fullest satisfaction that they are not the facts of observation ; that they are mere unusual, extraordinary matters, which, from their infrequency and being out of the usual course of things, become the marvellous points of communication. But even the authors of these wonderful tales do not tell us, that the labour with the savage is without pain. From these persons we collect the information, that, among this unsettled sort of people, there are no persons, male or female, devoting themselves to the obstetrick art. If this be correct, whence is their information ? Have they made it a matter of private enquiry with the individual Squaws ?

Are there no instances of preternatural presentation among the Calabrian societies ; such as that of the arm, back, or belly ? Would these too be without pain or difficulty ? These tales of uncivilized life are told to us in a most uncivil manner. They are an indecorous attack on the understanding of every man.

The savage, the negro, and the poorer sort of peasantry, are in their condition nearly similar. They present not dissimilar phenomena to the eye of observation. And with them there is naturally, and originally, in relation to the commencement of labour, an equal degree of difficulty, pain, and danger. Nature, in the general, is upon the same scale, and is safe ; I speak of the negro, the peasant, and the savage. Mis-

chief is the result of rude and clumsy art, in the hands of adventurous ignorance.

In fine, we have no authorities upon which we can, with safety, proceed in our investigations into the state of parturition, amidst savage life. And what are we to think of the narratives of men, totally without the means of instruction, in respect to the affairs of the woman of the forest, when a lecturer can publish to the world, "that the labours of the brute are not generally attended with pain, or difficulty." Have any of these intelligent travellers been at the couch of the lion, or the lair of the wild-hog, the den of the wolf, or the hole of the fox? The female horse may die with her foal in her matrix; there are birds of prey and quadrupeds, fierce and carnivorous: who is to save from the talons of the one, and the jaws of the other, the objects of our research? In speaking of the brute, the Doctor cannot certainly allude to such as browse our meadows, bound along our plains, or to the trusty animal that faithfully guards the shepherd's flock. Such news, bearing the stamp of authenticity, would be joyous tidings to the farmer.

The Abbot Raynal wrote a book, the object of which was to prove to the credulous world, the deterioration of all animals, even man, in this western world. From whom did the Abbot get his information? From travellers. From

whom do learned gentlemen get their information? From travellers.

From the most correct view I can get of the subject, I am inclined to believe, that natural labours (all labours are natural in themselves, and only unnatural in their circumstances) are as frequently interrupted by rigidity of the os uteri, with or without inflammation, among the laborious negroes of the farm, or the peasants of the thatched hut, as among the most delicate ladies, who live amidst all the luxuries of civilization and refinement ; indeed, much more so. My opportunities, on this head, are not very limited.

It avails nothing to say, that we have departed from the life of the savage, and that an equal departure from the simple dictates of nature, in search of a corrective, is necessary. With the savage and with the civilized woman, the mechanism of labour is the same. There is a certain compound resistance to be overcome by the co-operation of given forces of expulsion. A change from the savage mode, to that of civilized life, does not, and cannot, alter the relation between this sum of compound resistance and these forces of expulsion. It neither abstracts from the aggregate of one, nor diminishes the efficiency of the other. If the one be altered, the other is equally altered. They are equally dependent on the same economy of the general whole.

Can we believe, with the lecturer, that "the man (a specifick term, by which an individual is put for the kind) of the civilized world, has lost much of his original strength," &c., and yet, that this man, thus plundered of his powers, is more subject to inflammation, &c.? Sir, what do you think of that philosophy which points out the lax fibre, the body with ruined energies, as the proper subject of rigidity, the very object eligible for the lancet? Can your ingenuity unfold to you, how the delicate lady, whose bed is down, and whose life is inaction, can be more obnoxious to inflammation, rigidity, &c., and better able to bear large abstractions of blood, than the wild savage, whose body, like that of the hardy rustick, is braced by exercise, whose blood is pure and rich, from a simple, yet substantial diet?*

But, to fill up the measure of this singular philosophy, the lecturer declares, that, although

* We generally find the women of the country more obnoxious to it (pain) than those of *cities*. *Museum*, Vol. I; No. 3, p. 280. *Dewees*.

And this is, I suppose, a logical deduction from the proposition, that "pain is produced by civilization and refinement!" Then the hard-working woman of the city, and the laborious of the country, are the civilized; and the delicate, *refined* lady of the city is the uncivilized woman. Brydone certainly did not add to the facility of birth with the Sicilian women, that they were also "savages." In the *Museum*, all the advantages are with the lady of the city. In the essay, she has lost every thing, "by civilization and refinement." See *Dewees*, p. 43, &c.

the man of the civilized world has lost much of his original strength, "the circular muscles, the heart and intestines, as far as we can determine, have lost nothing of their primitive powers." Now, if the circulation, which depends on the heart, and the digestion, the office of the alimentary canal, be in the vigour they were in during the days of our first parents, the man of the civilized world cannot have fallen off much. Where there is a vigorous circulation, and strong digestive powers, the muscular energies of the body cannot be low. I allude to those muscular energies (I believe the long, straight muscles to be concerned) which qualify the Hibernian, in proud contempt, to poise his shillelah, the nervous Englishman to shake the very walls of the theatre of pugilism with loud huzzas, when he has pushed his unequal antagonist on some unresisting spot; which qualified the proud Roman to project the coit, or throw the javelin; the alert Greek, covered with sweat and dust, to carry off the prize in the gymnastick circles; or brawny American to turn, in coarse exultation, from his fallen competitor.

The doctrine of the circular muscles is new; I am not prepared to receive or admit it. I believe it to be wholly inadmissible, from our present anatomical knowledge, and social observation.

Dr. Dewees talks as lightly and familiarly of labours "without pain,"* as if they were the objects of daily observation. In the examination of matters of science, we are necessarily restricted from all loose modes of expression. We are not to be indulged in tropes, and figures, and flowers of rhetorick, by way of decoration to our subject. I am a good deal sceptical about labours without pain; and, when I look at the immense volume of female experience, as it is unfolded in every age, and every nation, I am led still farther to doubt. The world should, at least, have furnished *one* case, where the throes of labour have been passed through without pain. I have not, as yet, read one honest account of such a case, except where the sensibilities were benumbed by stupor, suspended by syncope, or annihilated by death.

The ancient doctrines of Boerhaave make but an awkward appearance in their new American dress: doctrines that enlightened science had committed to the stream of time, that they might

* "And of women delivered without pain, it would be idle to cite them, as they must occur in every man's practice." *Dewees, Essay*, p. 43.

I have never met with one of these labours without pain; and, to my recollection, neither Smellie, nor Hamilton, nor Denman, nor Baudelocque, nor La Motte, nor Levret, nor Lloverjat, nor Walker, &c., &c., speak of such a thing, where the body retained its sensibilities. These men write of easy labours with little pain, and yet their practice was not among savages. Were these painless cases of the *Essay* among civilized people?

no longer reproach the understandings of professors. That pregnancy is a state of disease, was taught by Boerhaave to his pupils, and commented on by Vansweiten, in his illustrations of Boerhaave's Aphorisms.* It is like many other discoveries that are now-a-days fallen on.

The first few pages of Dr. Dewees's pamphlet excited my surprize ; but, when I arrived at the article upon blood-letting, I was overwhelmed with astonishment. He gravely tells us, that in labours attended by rigidity of the os uteri, with or without inflammation, with irregular contraction, &c., the utility of the lancet originated in his own observation, and with himself ! That " in diminishing pain, disposing the os uteri to dilate, the external parts to unfold, &c., blood-letting originated, as far as I am acquainted, with myself."† Are all preceding writers

* " Morbi gravidarum." " Postquam gravida est fœmina, plurimis afficitur malis ex sola graviditate oriundis. *Boer. Aph.* 1293.

" Facile patet, hic tantum agi de illis morbis, qui a graviditate, tanquam causa, pendent, non autem de illis, qui gravitatis tempore contingunt quidem, verum aliis causis originem debent." *Vansweiten.*

With parity of reason Boerhaave might have said, that the function by which the blood is oxidated is a state, a gradation of disease, because, in the first acts of respiration, children utter cries of apparent alarm and pain. Certainly such things can only point out to us the present general state of suffering humanity.

† Essay, p. 63.

on general midwifery silent on this head? Or is the Doctor's reading limited to his own writings? Each writer must speak for himself.

"In lingering labours, when the parts are *rigid*, if the patient is of a plethorick habit, with quick strong pulse," says Smellie, "the contrary method (opposed to cordials) is to be used, such as *venesection*, antiphlogistick medicines,"* &c. Here is the doctrine recognized by old Smellie, an unfashionable writer it is true, in all its principles. He advises *venesection* in rigidity of the softer parts. For what purpose? Surely "to dispose them to dilate, to unfold" before the head of the child, to prevent or cure, as the case might be, inflammation. He could not, nor could any other rational man, suggest to us blood letting, merely for the ceremony of the thing. He must have intended it to be in its extent commensurate with the circumstances of the case.

Thus, where Baudelocque remarks, that "at bleeding made to the purpose (a happy expression), with emollient, mucilaginous injections, the warm-bath, &c, * * * * can relax the *rigidity* of the neck of the womb, and render its di-

* Smellie, Vol. I, p. 221.

† "Une saignée faite à propos, des injections émollientes et mucilagineuses, les bains * * * * pour affoiblir la roideur du col de la matrice, et en faciliter l'ouverture." Tom. I, par. 1145. Baudelocque.

latation easy." He does not restrict us to any given quantity. There is, however, but one thing to be understood. The bleeding is to be "*à propos*;" suited to the circumstances of the case, as to extent, and done in time. What reasonable man could have said more?

In the same strain, we observe the opinions of Denman to run on "fever and local inflammation" (rigidity with inflammation). This excellent accoucheur says, that "It does not seem necessary to bleed every patient on the accession of labour, and for some it would be highly improper. But whenever the feverish symptoms become violent, it (blood-letting) is, I believe, *universally* proper; the quantity of blood taken away, being suited to the degree of fever, and to the constitution of the patient."* If the fever arises from local inflammation, as we understand from the head of the section, and as there must be *pain* where there is local inflammation producing fever, he assuredly prescribed blood-letting "for the diminishing of pain."

A little farther on, a few pages only, the same judicious man adds: "For the prevention of such difficulties as may attend the first act of parturition, in those who are advanced in age, we have been advised to order frequent and small bleeding towards the conclusion of pregnancy."

* Denman, Vol. II, p. 50, 59.

For what is this advice ? For no other purpose that I can devise, than “ to dispose the rigid os uteri to dilate, and the external parts to unfold.”

To those already adduced, I will add Hamilton, who tells us, that in “ crampish spasms of the belly,” or viscera within the belly, we are to have recourse “ to *venesection*, glysters, &c. And in the subsequent paragraph he subjoins, that “ inflammatory diathesis, in young subjects of strong rigid fibres, and plethorick habits, must be obviated by *venesection*, repeated glysters,”* &c.

It will now appear that blood-letting has been used, by those writers that are well entitled to a place in the library of every medical gentleman, “ for diminishing pain, disposing the os uteri to dilate, the external parts to unfold,” &c. ; and that these writers recognize and enforce the doctrine of blood-letting in its fullest extent, so far as rational men can go. The doctrine, with its principles, is all that can be contended for. The application must rest with the judgment and discretion of the practising accoucheur.

In the quotation of the 1960th paragraph of Baudelocque, there is great injustice done the learned Frenchman, as it is cited in the thesis. Why were the “ proper methods,” so hastily

* Hamilton, p. 156.

passed over? Are they undeserving of notice? What are those "proper methods," that are to have place before the accoucheur is to think of the cutting instrument? Turn to paragraph 1145, and you will see it to be nothing less than what the learned lecturer claims as his discovery, "blood-letting," &c.

It may not be amiss to remark, that there are two states of unyielding os uteri: the one from an inflammatory disposition, the other from a cartilaginous state. How the lancet will succeed in real cartilage, we are scarcely prepared to determine. The bistoury should be the last thing thought on by the accoucheur. This is Baudelocque's doctrine.

That, by a loss of blood to any extent whatever, the practitioner will prevent the disease called the "swelling of the lower limb of the lying-in woman," Dr. Dewees will not again assert, when he shall have taken a better and nearer view of that disease. It is as common in feeble, exhausted habits, as in robust, plethorick ones. I write from observation. See also Charles White, of Manchester, Denman, &c.

Of cicatrices I have no experience; but I can have no very exalted opinion of that remedy, which, after several repetitions, leaves the case to be relieved by the forceps. The lancet would be proper without question, so far as irritation

and inflammation might be threatened from the effects of long-continued, violent throes.

From what has been premised, I deduce the following conclusions :

1. That pregnancy, whether with the savage or civilized woman, is a state of nature, and parturition an act of nature, but subject to interruption by disease.

2. That the doctrine of pregnancy being a disease is inconsistent with the order of nature, at variance with common observation, in itself indefensible, and of the school of Boerhaave.

3. That the rigid os uteri, with or without inflammation, is a rare occurrence, there not being more than ten cases in a thousand of ordinary practice, where the accoucheur would be called on for the aid of his art.

4. That blood-letting has been considered and prescribed as a remedy, "for diminishing pain, disposing the os uteri to dilate, the external parts to unfold," &c., by Smellie, by Denman, by Hamilton, and by Baudelocque.*

* "I am aware that this remedy (blood-letting) is not a new one to a certain degree ; but, I believe it has never yet been advised nor used in the copious manner, nor regulated by the morbid phenomena" (rigidity, local inflammation, unyielding disposition of the soft parts, &c.) "of child-bearing, which have been mentioned." *Rush. Rep.* vol. VI, p. 20.

I will close this letter, with a short admonition to young practitioners. It would be well for gentlemen, when they enter the room of a lying-in patient, to carry with them, not their lancet, for I have understood that those who wear the sword are apt imprudently to use the sword, but a good stock of composure and patience, and confidence in the resources and abilities of nature; and, above all, a sound, discriminating, well-instructed judgment. Thus provided, I think that I can assure them, they will seldom leave the room with feelings ungrateful to themselves, or a reputation disadvantageous to the art of the accoucheur.

In my next speculation, I shall confine my pen to the mechanism of labour. I shall endeavour to demonstrate, that if the lancet will relax the softer parts through which the child is to pass, it will also reduce the efficiency of the throes, necessary to the detrusion of the child; and, that

“ This remedy (blood-letting) is by no means a new one, in labour; but employed for the express purpose of diminishing pain, and subduing the various species of rigidity just spoken of, and carried to an extent that will ensure these objects, that is, diminishing pain, disposing the os uteri to dilate, the external parts to unfold, &c., originated, as far as I am acquainted, with myself.” *Dewees, Essay, p. 62.*

If, sir, you will compare these two paragraphs, I think you will coincide in opinion with me, that, were they in different languages, the one might, with the strictest justice, be considered as a free translation of the other. Did Dr. Dewees ever read Dr. Rush?

where one ordinary case will be benefited, one hundred will be injured, by the lancet.

I am yours, &c.,

JOHN B. DAVIDGE.

Baltimore, March 25th, 1807.

From Dr. Davidge to Dr. Barton.

DEAR SIR,

IN my last I gave a promise, that, in this my subsequent speculation, I would restrict my pen to labour; to the symptoms proper to labour; and to the forces by which it is effectuated: that I would endeavour to demonstrate labour to be morally and physically painful; and this, from the present condition of humanity, to be of necessity, and not of contingency. I moreover promised, and in this the promise chiefly consisted, to demonstrate the inadmissibility, in ordinary, natural labours, of the use of the lancet; that, in ninety-nine out of the hundred ordinary labours, it must be productive of unpleasant consequences to the parturient patient, and, when copiously used, it must retard labour. I now take up my pen to fill up the measure of my promise.

We will suppose the woman to be entering on her ninth month, as, until the first ten days of that shall have elapsed, there is no symptom of approaching labour. Within about three weeks of the period of actual parturition, the woman begins to perceive a subsidence in the epigastrick and hypochondriack regions. This subsidence is not a mere chimerical deception of the sense of the woman ; it is an actual change, and becomes more and more the subject of regard, until the period of confinement. This subsidence has been a source of serious contemplation to the woman ; she has mistaken it as indicative of the death and waste of her infant. With some accoucheurs it has been a subject of idle animadversion, being construed into the collection and dispersion of wind in the stomach and bowels ; whilst with others, better trained in the school of observation and experience, it has constituted a part of the uniform design of nature.

To those who are acquainted with the regular evolution of the gravid uterus, I need scarcely remark, that this evolution is in divisions ; that of these, the fundus may be considered the first division, and is the first in evolving ; the corpus the second, and has the second place in suiting itself to the increased dimensions of the child ; and the cervix as the third, and the last in yielding to the growth of the contained fœtus. This last division does not lend its aid until towards the eighth month, from which time it in regular

progression evolves to meet the expanding dimensions of the fœtus.

When this is nearly completed, the child actually occupies a lower part of the pelvick region than during the sixth and seventh months ; and, about the completion of this evolution of the neck of the uterus, the evolving action of the abdominal muscles begins to change, and give place to the powers of expulsion resident in these muscles, and the longitudinal muscular fibres of the uterus. The uterus, from the interest it has in the general sympathies of the system at large, commands into its private services the aid of the neighbouring muscles. By an absolute impress of foreign agency, in co-operation with its own powers, it begins and carries on the function of expulsion ; and, in the preparation of these muscles for more serious action, we discover the first cause of the first premonitory sign, the subsidence of the abdomen, of approaching labour.

At full time, when nine months shall have completed their round, the throes of child-birth come on. They may fortuitously be provoked into untimely action, and produce abortion, or miscarriage ; but their legitimate term is generally conceded to be that of nine calendar months. If it were necessary, in addition to what I have said in my former letter on this subject, to adduce evidence in attestation of the fixedness and

uniformity of the term of labour, I would invite your attention to the highly interesting fact, that, if a fœtus be extra-uterine, the throes, at full time, will come on with the same force and regularity as if the fœtus were within the uterus. Here we have another argument strongly militating against the preposterous doctrine, that "pregnancy and parturition are grades of disease." The throes to expel an extra-uterine fœtus will not be argued to be derived from the stimulus of mechanical distention operating on the accumulated excitability of the uterus. We assuredly deduce these throes from the laws of the constitution of the mother ; for, in this case, the child is not within the womb, and, of consequence, can by no luxuriancy of fancy be supposed to excite into action that organ.

The term proper to the maturity of the child being elapsed, we discover farther and more decisive marks of labour to present themselves. But, before I proceed to the physical properties of the womb, I must say something on the moral necessity of pain.

What I mean by the moral necessity of pain, it may be proper, in a succinct manner, to explain. Notwithstanding the general sympathies of the body may, and to my apprehension do, grow out of the common constitution and economy of body, and would, without the transgression of man, have been attended by plea-

sureable sensations in the various operations of which they are the source, yet I find no difficulty to conceive that those sympathies, in their laws the same, may be accompanied by unpleasurable sensations in the functions or conditions to which they give origin.

In the first period of conception, upon the supposition that Eve had remained in her original paradisaical estate, I can conceive that all the associated actions would have afforded pleasure ; that the very associated action of the stomach, which, in the present condition of woman, causes distress, could, under the state alluded to, have produced happiness.

The body has been, for some time previous to conception, accustomed to have separated from its common circulating mass a peculiar liquor to a given quantity. Upon conception in the general, this particular excretion (the menses) ceases to be thrown off ; the body no longer, from its altered condition, throws off this excrementitious fluid, no longer can it need the same supply of aliment ; and hence is the general inappetency of pregnant women for food during the first two or three months, until, by the bulk of the child, greater demands shall be made on the general constitution, and, of course, on the stomach.

From this I wish it to be understood, that the sickness and uneasiness of breeding, as it is usu-

ally phrased, is a moral consequence, while the inappetency for food is, from a law of the female habit, under the control of ovarious influence. Therefore, when we speak of the diseases of pregnancy, I hope that it will be always conceived, that we do not have allusion to this constitutional association between parts, nor to the distress consequent on this associated action, which, in itself, in a higher or lower degree, howsoever opposed by human skill, is inseparable from the present condition of humanity, and forms one among the many irrefutable facts on which rests the authenticity of the bible.

CASTELLIO'S VERSION.

"Deinde ad mulierem: Ego te multis doloribus, inquit, aerumnisque afficiam, tu natos *cum dolore* paries." *Gen. cap. iii. v. 16.*

From this serious part of my letter, you are not to anticipate a formal discourse on theology. This excerption from sacred history is at present necessary to, and may have an advantageous place in, our discussion. Were it a light matter which at this moment solicits our attention, I should most sedulously have avoided an appeal to that authority, the sacredness of which renders it inaccessible, either in language or writing, except when in our solemn, serious meditation, in the stillness of spiritual contemplation, we approach, with a trembling dread and awful apprehension, to enquire into its momentous doctrines, to hear the great ever-living En-

tity unfold, in justice, wisdom, and celestial dignity, his irreversible purposes to deciduous man!

It is palpable, and bears with irresistible conviction on the mind, that the above address to Eve was not directed and limited to her as an individual; it was addressed to her with a meaning diffusive and general, terminable only by the limits of her descendants; as woman; as the mother of all human females.

This heavy denunciation was fulminated against woman, in the same spirit, under the same circumstances, and for the same high crime, as against Adam was launched the terrible menace, "*pulvis es, et in pulverem redibis.*"

This was uttered against Adam as man, the father and head of all living. Both were levelled against our kind, and not restricted to early ages, particular nations, or certain individuals. A few men, by miracle or special favour, have had, in relation to themselves, this malediction rescinded, but we know of no women or race of women, savage or civilized, to whom the curse does not extend, "with pain shalt thou bring forth children." When men cease to be mortal, women will be blessed with painless births.

The text in the English version is very obscure and indefinite; I therefore have preferred that of the learned Castellio, who, in the opinion of the most able criticks, stands among the

foremost in Hebrew literature. And it affords me great pleasure to find that the sense of Castellio is confirmed by the Greek* and French.†

Superadded to what this laborious Hebrician gives in the text, he assures us, in his foot-note, that the words, strictly translated, are “est dolor pariendi.” And to it, I am sure, all women, who have borne children, and are in the possession of a sound mind, will most willingly subscribe.

Dr. Dewees indulges in a most curious annotation on the text as it stands in the English version. He most gravely labours to prove, that the word “sorrow” does not, in any part of the sacred writings, mean an uneasy corporeal sensation. It would have been great gratification to all biblical scholars, had the Doctor dilated a little more on the general sense of the text, and given us a full commentary on “I will greatly multiply thy sorrow and thy *conception*; in sorrow shalt thou bring forth children.” Perhaps it would have appeared, in the course of his discourse, that woman, in consequence of her defection, had become more prolifick; for to mul-

* “In the Greek version, or Septuagint, the word *sorrow* is rendered λυπας, which signifies pains or sickness,” &c. *Vaughan. Med. Rep.* vol. 6, p. 31.

† “Dieu dit aussi à la femme : je vous affligerai de plusieurs maux pendant votre grossesse ; vous ne mettez au monde des enfans qu’avec douleur.” *French Version.*

tiply "*conception*," is without doubt to increase fertility.

What can be inferred from "I will greatly multiply thy conception," &c.? Nothing, but that the whole is incorrect, and a stupid blunder of the translator. The Doctor says, that it was not a punishment for transgression, but the mere result of accident or civilization. And yet the very version, and every other version, on which he offers his annotation, expressly declares, "because thou hast done this," &c., &c.

The tonick and clonick powers of the uterus, together with the contractions of the abdominal muscles and diaphragm, are the only actions alleged and recognized, by the concurrent authority of accoucheurs, as contributing their agency to the expulsion of a child from the gravid uterus. If there be others, I am unacquainted with them. All voluntary effort is both useless and improper.

THE TONICK POWER.

It appears to be understood among accoucheurs, yet not with the full and expressed consent of anatomy, that there is in the uterus a double set of fibres; the one circular, the other longitudinal. The circular fibres are diffused equally throughout the uterus; its fundus, its corpus, its cervix. In these fibres is resident

the tonick or elastick power. It perpetually urges the gravid uterus to recover upon itself, as the French would speak ; and no sooner do the throes, co-operating with the circular fibres of the fundus and corpus uteri, discharge the liquor amnii, than the whole of the walls of the uterus, the circular fibres of the cervix, with those of the other parts, close about the child, and in this state would the hapless fœtus, immured within its narrow habitation, remain, until death and putrefaction should waste it away, were not the revival and reiteration of the throes to impel it to the world.

The circular fibres of the cervix are forced into obedience by the superior power of the throes, of which I shall presently speak. The tonick or elastick power never ceases to act when once called on by the laws of the constitution, and provoked by a source of distension within the cavity of the uterus ; and no sooner is the child expelled with its appendages, than this tonick power gathers the uterus up nearly to its original dimensions. Of this tonick action the woman has no consciousness : it perpetually urges, yet without pain.

“ 1. That the circular fibres may contract to almost any degree, without being attended with pain. 2. That their contraction alone, however violent, does not forward the child. 3. That they do not possess the power of alternate con-

traction in the same degree as the longitudinal fibres ; and, that they may exert this power, it is necessary at first to have them distracted by some force or other," concludes Dr. Dewees, from previous facts or propositions.

Permit me respectively to invite your attention to these words from the essay of your lecturer, and impress your mind with their value : I intend, from them and their consequences, to establish the important fact, that the lancet, in no *ordinary, regular* labour, can afford the practitioner any possible aid, but must of necessity embarrass the natural progression of the parturient act, and disappoint the accoucheur who has unwarily trusted to it.

In no instance whatever has a child been delivered by the tonick power of the womb. In the case recorded by Levret, and the few others collected by writers, although the child was delivered after the death of the mother, its head must have been in the smaller basin of the pelvis, that is, without the os internum, and in the vagina, otherwise it is very obvious, from what Dr. Dewees writes, and every other man must believe, that it must have remained with the mother.

THE CLONICK POWER.

This power of alternate contraction and relaxation, and acting at certain periods or inter-

vals, is a property of the longitudinal fibres of the womb, associated with a synchronous action of the diaphragm and abdominal muscles. The abdominal muscles and diaphragm are brought into service by a constitutional sympathy between them and the uterus, such as obtains between them and the stomach in paroxysms of excessive sickness. These alternate contractions and relaxations, or clonick actions, constitute the real throes or pains of labour. They are uniformly, except in stupor, accompanied by an uneasy corporeal sensation, and hence are termed throes or pains. The word throe itself is from the Saxon term, to suffer. No throe can have place without the consciousness of the woman, except, as above remarked, under an affection of the common sensibilities of the body; and, without a throe, the gravid uterus can never disencumber itself of its burthen.

In every throe there is more or less of an uneasy sensation of the loins, haunches, or uterine region. At times, the lower extremities are distressed with most unpleasant feelings. In cases of convulsion during labour, the convulsive affection simulates very much, in its periodical recurrences, the genuine labour-pains.

From the antecedent premises, I deduce the fact, that, as "the tonick power, however violent, does not forward the child," and as there is no third agency alleged or recognized by phy-

siologists or accoucheurs in the expulsion of the child, there cannot possibly be an act of parturition without throes or pains.

Assuredly the most enthusiastick theorist, who dreams out his cases in his closet, is not prepared to admit or maintain that throes can be present, and yet the woman feel no corporeal distress. This were an absurdity too monstrous for any sane mind to contemplate, but with disgust and abhorrence.

I shall now proceed to the examination of that philosophy which instructs in the abstraction of blood from a general system of vessels, in order to relax a particular set only. This general system, we may observe, goes off from the same heart; is mutually in its parts dependent, and between which parts there is a constant reciprocation of good offices, or injuries, with a continued, uninterrupted circulation of the same common mass of fluid. The circulation through this general system being continuous, and, as it were, in a circle, any quantity of blood abstracted must immediately, and in the ratio of the diameter of each vessel, influence each and every part of the general whole equally.

If any loss of blood will relax the os uteri, vagina, and loca muliebria* (I speak of these parts

* *Leci muliebres, vel loca muliebria*, are by some classick writers preferred, when they speak of the genital parts of the human female, while they rather restrict the expression *puden-*

in their ordinary state, free from inflammation, rigidity, or spasm, such as ninety-nine of a hundred labours will be the case), will it not likewise prostrate, in a higher or lower degree, the expulsive forces of the uterus, abdominal muscles, and diaphragm, and equally in their relation? It must unquestionably, and leave the resistance to be overcome, and the powers of expulsion, in the same relation to each other that they were previously to the use of the lancet.

Whatever be the quantity of blood, its relative effects must be the same. If even the woman be reduced to syncope, the child remains in the womb; for where there is syncope, there can be no clonick action, and the child will not be born until the woman shall be restored to her powers.

During utero-gestation, if a woman has incautiously, by excessive exercise, roused the heart and arteries, or the circulation has been accelerated by intellectual emotions, or a fever has been kindled up, the accoucheur abstracts blood: but for what? to relax the parts through which the child is to pass in its birth? by no means. His object is to bring the body more or less to a state of relaxation or syncope, and thus

da to the genital parts of the female brute. But, perhaps, they are equally proper, and may be used according to the taste or choice of the writer.

to lower the powers of expulsion, to quiet the parturient throes, and save his patient from miscarriage. If, then, during utero-gestation, we are to prevent premature birth by the abstraction of blood, why, and how, are we to facilitate parturition by the lancet at full time? Will not the effects generally be the same? Undoubtedly so.

We think correctly, when we believe that nature has as wisely ordered the relation of her powers in the act of parturition, as she has regulated the graduation of the stimulus of the blood to the irritability of the heart. Not indeed to a mathematical balance, but, according to her own liberal scheme, admitting of some extravagance and aberration. Therefore, every light disease, or improper interference of art, cannot throw her from her design.

Any given quantity of blood, whether from the arm or uterus, should, to my understanding, affect the general system, and, of consequence, every part similarly; and a relaxation of the body, or complete syncope, must be equal, in what manner soever induced. Hence I infer, that no woman should die from flooding; at least without being first disburthened of her load: nor indeed could she possibly die of flooding before the birth of her child, if the doctrine, embraced by the principles inculcated by the friends of syncope being the immediate path to easy and

rapid labour, were founded in the laws which regulate nature.

I write from observation ; I am an accoucheur, and have witnessed the effects of bleeding in ordinary labours : it is true, not in my own patients, except such as have become so secondarily. As to labours attended by morbid phenomena, I have already spoken of them in my first letter. The use of the lancet, in such, is recommended and enforced almost by every writer on general midwifery, for fifty or a hundred years back. My present letter only relates to ordinary labour, and neither of them to preternatural or laborious labour.

When we talk of relaxing the vagina and external parts by the lancet, in common labour, we use a sort of language too mechanical, and in no respect applicable to the affair of which we speak. This relaxation, as we are pleased to term it, of the soft parts, is a peculiar and inscrutable evolution or unfolding, which may suggest to us a high veneration for that wisdom which ordered the plan of the parts and their functions ; and certainly we may be content with the knowledge of the fact, without entangling ourselves in useless researches after physiology ; researches that will ever be unsuccessful, and will only serve to teach us humility.

At best, the science of physiology is a science of conjecture. We know the organs, and we

learn the result of their operations ; but of the causation, the physiology of the thing, we know nothing, whether we speak of the uterus, the liver, the brain, or any other organ.

This same unintelligible, mechanical language has been as fruitlessly applied to the explication of the evolution of the gravid uterus. Like the gravid uterus, the vagina and external parts evolve by the particular physical properties of the parts themselves. These properties or laws are a part of the wonderful economy of nature ; they are called into service by powers of the female constitution, and should have taught professors that parturition is not a “ grade of disease.” So soon as disease fully takes place, this natural evolution ceases, and art must open its resources.

I may add, before I close, a few animadversions on the general though reprehensible practice among accoucheurs of artificially supporting the perinaeum during the last act of the parturient operation, as if nature called more for adscititious aid in one step of her course than in another. Can there be any thing more gross and exceptionable in the eye of enlightened philosophy than the supposition that more aid from art would be required to facilitate the passage of the child through the os externum than would be demanded for its passage through the os internum ? Surely gentlemen are not serious in their advocacy

of this relic of folly, this remain of mistake. It were as rational and becoming to renew the pleadings and reasonings for the baths and macerations to relax the ligaments of the pelvis that it might enlarge in its diameter and afford facilities to the descent of the child, as to appeal to art to aid its egression from the Vagina.

Are those efforts to sustain the perinaeum and save it from laceration the dictates of experience—the doctrine enjoined on the practitioner by the language of observation? certainly not. If left to itself the evolution of the external, soft parts will go on as regularly and safely as that of the mouth of the womb. How difficult it is for the accoucheur to learn a negative science, to know that in the general he has no duty to perform beyond the simple acts of receiving the child, securing the cord, and taking the afterbirth from the vagina! An humble office it is true, yet one with which modest intelligence is content. The man of science reserves himself for scenes of danger and trial, where nature is forced from her ground, or is failing under a load of difficulty. Then by a dextrous application of the important rules furnished by his art, he comes to her relief and guides her through her perils.

I am yours, &c.

JOHN B. DAVIDGE.

Baltimore, April 10th, 1807.

From Dr. Davidge to Dr. Smith.

DEAR SIR,

TO your polite favour I should have returned an earlier answer, but for a disability of my right hand, occasioned by a slight and transient casualty. That you "will derive pleasure" from a correspondence with me, is both flattering and gratifying; but when you demand "improvement," you will permit me to admonish you to make your drafts on other banks than those over which I have control, lest, from a deficiency in credit on my part, your bills return dishonoured.

The *nosology* of a wounded artery is interesting and highly important. Its range in consequences, in all serious, indeed trivial wounds, is great, and well entitled to the attention of the surgeon.

You will not, my friend, be startled at the sound of the word *nosology*, the object of which is, an inquiry, you know, into the *nature*, the *symptoms*, and *consequences* of diseases; except, indeed, you have become a convert to the new philosophy of the day—except you consider nosology as an unfit subject for the present enlightened period, believing that in dark periods of ignorance, when gross night overshadowed the

schools, such stupid themes had an apology in the dull, unimproved state of the times. But now that a beam of new light has awakened the medical world from its long and deep slumber, you may imagine that a subject more ennobled by novelty, would be less unbecoming. Be it so ; and let my present mention of it find an excuse in my early prejudices. But I widely miscalculate if I do not in some little speculation, which, if I can rouse myself from my indolence, I intend to send you, *demonstrate* that where nosology ends, medicine ceases to be a science. But to the subject of the present letter, lest you think that I have designedly strayed to hunt up the errors of great men.

A sword passes through the fore-arm, and some small arteries, with a larger one, the radial, ulner, or interosseous for instance, are divided. The blood pours furiously out for a time, but is, by coagulation of the effused fluid, and the coarse means externally applied by the family, or some generous neighbour, stanchèd. The surgeon arrives, and finds all things quiet, although in homely order. He feels reluctant to open anew the vessels by breaking up the dressing ; and begins to hope and persuade himself that the hæmorrhagy will not recur. How vain and illusory !

In a day or two the hæmorrhagy breaks out again, and is again checked ; and thus I have

seen matters proceed, for two and three weeks, until the hand, wrist, and fore-arm have been completely injected, and the patient exhausted to the extremest degree. And what does all this arise from? From one of three things.

The surgeon has read, or heard, for all our surgeons do not read, that if the lips of the wound in an artery be coaptated, whether by good luck or artificial means, they will unite after the manner of a vein, the gluten being poured out by the vasa vasorum. Or, if the artery be completely divided, it will contract up to its first branch. Or, a coagulum without and within, will form, and serve as a stopple to the artery. And in this last, countenanced by the doctrine of the vitality of the blood, he begins to imagine to himself, or talk to those about him, how this coagulum forms its new vessels, and how they stretch out towards the walls of the artery, and how, by uniting with the sides, the plug is secured. Here are three sources of error in the surgeon, and danger to the patient.

I hold it as an axiom in the surgery of a wounded artery, that no hæmorrhagy from an artery will cease, except the sides come into close contact and union.

In the smaller arteries, I conceive, that, by the effused and coagulated blood without and about the sheath of an artery, its sides are forced into union; that is, that they come into con-

tact; that the vessels of the internal coat are excited, and take on that action which brings about what surgeons call the union by the first intention.

What is it in those smaller vessels which, after the patient is put to bed—his heat restored—and the re-action established, causes the recurrence of the hæmorrhage? Plainly and palpably the increase of the current of blood forcing open the mouths of the divided arteries, in opposition to the mechanical pressure without; not the heat or any other cause relaxing the contraction of the extremity of the arteries.

And what is it in the larger arteries which causes them again and again to pour out their blood? To my understanding, the putrefaction and melting down of this coagulum, to which the surgeon unhappily trusts. And as often as the coagulum forms, the hæmorrhage is stopped; and as often as it begins to putrify, and melt down, and fall away, the hæmorrhage returns. If there be a coagulum within at all, it is too insignificant to be noticed; and, it will ultimately deceive the surgeon. I do not speak from conjecture; nor do I rest on the highly respectable authority of Mr. John Bell, who appears to be destined, in his luminous course, to give light to the universe of surgery. But, as I had the pleasure of mentioning to you, I have had an opportunity of dissecting two stumps, at the dis-

tance of thirty odd days from the period of operation. In which, when the vessels were neatly and fairly exposed, they were found completely cylindrical and open down to one eighth of an inch of the very extremity. The wall that closed up and formed the end, was but very little, if any, thicker than the sides measured at any place above in the most healthy parts.

Here was no coagulum, no plug, no conversion into ligament up to the first branch. But the lumen arteriæ was open and free down to the very point where the ligature closed upon the walls. This dissection was nearly forty days from the period of operation, and yet the artery had all the characters of health—its coats having no marks of thickness in any part.

There were several medical gentlemen present during this dissection, and among them my ingenious and learned colleague, Dr. Cocke ; a gentleman well known to you for his politeness, suavity of manners, and intimate knowledge of the healing art ; and whose mind, we may suppose, was well prepared to detect any circumstance that might militate against the doctrines of the schools in which he had been educated—those of London and Philadelphia ; and in those schools the doctrines of contraction and coagulation have certainly not been without friends.

But from what I have advanced you are not to infer, that I deny the possibility of a union of

the sides of the vessel, up to the first branch. I admit it ; but not from contraction, nor coagulation, but from the walls of the artery being forced into contact by mechanical agency ; and this would happen not only up to the first branch, but up as high as the mechanical force acted.

At first, immediately on tying of the vessel, there may be, and really is, a coagulum within—yet this soon melts down, and is mixed with the common circulating mass. And here the surgeon is deceived. He takes the column of jelly, in a few weeks to be melted away, for living substance, filling up the tube.

Yourself and every other gentleman accustomed to the knife, knows, from repeated observation, that when an artery is divided, it retracts, and draws itself up within its cellular sheath ; but the contraction of a divided artery even in a slight degree is to be conceded with caution. If an artery is to be provoked by adventitious stimulus, it will contract ; and this so long as its excitability shall be exposed to foreign stimulation. But it is not on this that the surgeon is to depend. It is uncertain, irregular, and of small compass.

When an artery is divided, lies exposed, and has its calorick with the fluid, which fills its vasa vasorum, to fly off, it is possible that its canal may be diminished below the natural state of its systole ; though this is too unimportant to be

brought into account ; and I am induced to consider the contraction of an artery an imaginary thing, equally illusory with the coagulum within the artery, the plug or stopple of which surgeons delight to speak.

Mr. John Bell says, " that an artery, in the moment of being divided, may be made to reunite, I hold to be an absolute fact, not because it is proved by the authority of those who have prevented aneurisms by a skilful compress ; for in such cases the aneurism being prevented, we are still left in doubt, whether the artery was actually wounded ! but the aneurismal varix ascertains the fact." vol. i. page 211.

Most singular indeed ! that an aneurismal varix ascertains the fact, that the lips of a wound in an artery adhere, if laid well together at the moment the artery is wounded. Or should we not say, that of all facts, the aneurismal varix gives the most decided proof that the wound has not united : for, in the aneurismal varix, the lips of the wound of the artery adhere to the fascia below, while those of the vein adhere to the fascia above, by means of the intermediate cellular substance. If the lips of the artery were united, there could no longer exist an aneurismal varix, as the current of blood from the artery which constitutes the disease, would be cut off by the closing of the aperture in the artery. Even Ulysses would not at all times pass the arrow

through the ring, were Minerva to leave his side.

My opinion is, that we should secure every injured artery by the regular compress or ligature, doing by art what must be done by accident, if the hæmorrhage cease; for I do not hesitate to lay it down as a grave and serious fact, that the walls of every artery must come into contact, and its lumen be obliterated, when a wound in an artery is successfully managed. I am fully aware, that many, and able surgeons, are of a different opinion; and that arteries are kept to show the gelatine up the tube, and consolidation up to the first branch. This gluten, no doubt, with all its appearance of commencing organization, would have been gradually carried away, had the patient lived but a few weeks. The serum and red globules, we all know, quickly run off, and after a time, as assuredly, the more tenacious or viscid gluten is dissolved. This we see when clots of blood are laid on healthy granulating wounds—when long compressed in the uterus, &c.

And the consolidation up to the first or second branch, is equally obvious. The walls are in contact; a slight action, approaching to that of inflammation, takes place, and a cord of more or less thickness, is left to deceive the surgeon.

The case, so frequently referred to, and so much confided in by surgeons, by Lambert, of

Newcastle upon Tyne, is far from deciding even the possibility of the re-union of the lips of a wounded artery, much less that it is to be generally expected.

When in a numerous series of experiments, one only is to appearance successful, we should be extremely careful how we extend our confidence. I am, in all these solitary instances of success, much inclined to doubt, and feel it my duty to guard myself from deception, by critically and severely examining into the circumstances of the case.

There are against Lambert's case many weighty objections. He tells us, and upon his words I shall take the affair, that, "a small steel pin, rather more than a quarter of an inch long, was passed through the two lips of the wound in the artery, and secured by twisting a thread round it, as in the hair lip. This was found to stop the bleeding," &c.

Whether the discharge of blood was arrested by the pin, with the thread about it, approximating the lips, and wholly closing the aperture in the artery, is to me very questionable. I rather conceive it to have been from the bulk of the pin and thread pressing, so as mechanically to close up the diameter of the vessel. And this pressure was undoubtedly increased by the binding up the arm: for in a wound so extensive as to expose the brachial artery at the fold of the

elbow, the consequent inflammation and swelling could not have been inconsiderable. The pressure must increase momentarily with the inflammation and tumefaction. And, in fact, if at first the anterior wall of the artery was not in contact with the posterior, it must have been so about the fourth day, the period at which "the wound was first drest."

This, my conjecture, is strongly corroborated, by the fact of the pin coming away with the dressings on the fourteenth day. Here it is certain that the anterior part of the artery must have been affected with high inflammation, and even gangrene and sloughing, or the pin could not have come away with the dressing. It otherwise would have remained for the surgeon to draw out, having previously disengaged the thread. It is clear, and beyond a question, that the front of the vessel mortified and sloughed. Can you conceive it probable, that in a vessel so small, and pressed on as this must have been, the anterior portion of its circle could inflame, gangrene, and fall away, and yet its lumen to be preserved, and a current of blood to continue down through it? If so, your conception is more sprightly and ingenious than mine.

But the surgeon says, that "the pulse was very little altered immediately after the operation," "and so strong and equal, that he had no doubt of the blood continuing to circulate freely

through it." What! is there no way but the great trunk for the blood to get into the radial and ulner arteries? Are there no anastamosing branches which might possibly convey it down, and thus support the pulsation of the radial or ulner artery? I have seen, and any surgeon may see when he pleases, the upper portion of the temporal artery beat freely and strongly when its trunk below has been absolutely cut asunder by the shoulder of the lancet; not surely by a continuance of the circulation through the trunk, but by the return blood through the anastamosing branches.

Superadded to the chance by the collateral branches, is it not possible, and indeed highly probable, that in this solitary case of success, the great trunk may have divided, high up in the arm, and of consequence a large portion of the blood would go off, and thus support the pulsation below?

That the diameter of the artery was completely annihilated, all the circumstances of the case tend to prove. And as to the pulsation below, I can readily conceive it to be kept up by the circulation through the anatomising branches, as already remarked, or the possible *lusus naturæ*, the branching off, high up the arm, of the radial trunk. And had the arm upon the death of the patient been injected, we have every reason to believe the canal of the artery would have been

found obstructed. Vide Med. Observ. vol. i. p. 360.

There is another circumstance of arterial hæmorrhagy, which at present solicits my attention. I allude to the frequent recurrence of hæmorrhage after the operation, for the popliteal aneurism, when the operation is performed in the front of the thigh. There is nothing in the nature of a femoral artery aneurismal in the ham, which should make it subject to hæmorrhagy. The fault, without doubt, must be solely in the manner of securing the vessel. I need not enter minutely into detail of the various methods of doing this plain affair—the ligatures of reserve—the metallick plates—the modes of confining loops, &c. They all tend more or less to break up the connections of the vessel with the surrounding parts. They deprive it of nourishment, by cutting off its channels of support. The very solicitude to secure the vessel, together with its being left continuous and undivided, has been the source of all the evil.

What could be requisite to produce hæmorrhagy in an artery, more than to have it insulated—cut off from all the neighbouring parts—bedded in pus and coagulated blood? Surely nothing. How could its private æconomy be maintained, when the very instruments of support were destroyed, when the blood could no longer flow to its bare and exposed walls?

And after John Bell had written so ably and intelligibly about a plain fact, which, to be understood, needed only to be pointed out, how could he suffer his transcendent pen to encumber itself with that mongrel, equivocal philosophy, that so unintelligibly discourses of “the artery being eroded by the foul pus?” The idea of the sides of an artery being eroded by foul pus, is so far below that clear and luminous philosophy which so vividly shines through every part of Bell’s great work—the greatest of surgical works—that I can scarcely consent that it shall stand in the splendid copy I possess.

What more need he have said, than that the vessel was insulated, and without support? Consequently its walls must die, mortify, and fall away; and thus would the blood burst out. To Bell we owe much in many things, especially for what he has written on the arteries. To what he has said about the manner of managing the affair, I can add nothing.

Baltimore, July 25th, 1809.

From Dr. Davidge to Dr. Smith.

DEAR SIR,

WHEN I did myself the pleasure of addressing to you my former letter, on the pathology of wounded arteries, I had not seen the work of Dr. Jones on that subject, and all I knew of the work, I collected from the desultory conversation I had with you. Not having seen the work, I could have no specifick objects against which I might direct my observations. The principles of Jones I conceived to be erroneous, and, at the moment, offered a few reasons, on which I thought them inadmissible ; I further added to those reasons, in my letter to you, and now having, by your courtesy, the work before me, I shall present, in full, my objections to his doctrines.

The Doctor has managed his subject with much ingenuity ; his language is easy, perspicuous, and rises to elegance ; the plan and order of his experiments are natural and unexceptionable ; but I cannot grant my approbation to the manner in which he has found it convenient to represent the various results of his numerous experiments. With the results, as they stand, I am particularly gratified. Nor have I the slightest wish to deduct from their force. I ad-

mit them all their influence ; but I cannot agree to the interpretation the Doctor has put upon them. I must assume the right of inquiring into, and minutely weighing the experiments in their circumstances ; I expect to show a preponderance different from that given by the Doctor, to the publick. Nothing is more palpable to my understanding than that, in his laboured work, Jones has ascribed more importance and efficiency to some of the circumstances than can justly be attributed to them ; and that he has overlooked the more immediate relations between them and the results.

If I can, without offering violence to the general tenor of the facts, or shading any one part to the disadvantage of another, satisfactorily explain to you that the Doctor has not been happy in his deductions, I shall consider myself fortunate ; but yet I shall do no more than justice to my undertaking.

In some place of Bacon's elaborate work he has remarked that, in the explication of any phenomenon, more causes than are adequate and necessary should not be adduced. This grave apothegm is the suggestion of a cautious and circumspect philosophy ; a philosophy luminous and observant. This caution arose out of a clear conviction, furnished by years of accurate and attentive observation, and it should ever be kept in view by those who may come before the pub-

lick, engaged either in the illustration of old principles, or the establishment of new hypotheses.

There is nothing more usual in the institution of experiments, for the ascertainment of any fact, than for the experimenter to enter on the affair with his mind more or less bent up to his purpose. In his closet he anticipates what he wishes to find in his experiments, and in his experiments he is sure to discover what he has already anticipated. We first enlist our prejudices and affections in behalf of our doctrines, and then the illusions of the circumstances not unfrequently flatter us, that our experiment has afforded the clearest verification of our hypothesis.

I do not premise this, to excite any undue prepossession against the deductions drawn from the experiments of Dr. Jones ; far from it : I wish these experiments to be open to a fair analysis and honourable discussion.

I would not trouble you with this criticism, had not Dr. Jones laid open anew the avenues of error, which had been so judiciously narrowed, and in a great degree closed by Pouteau and Bell. For, although to surgeons of experience and accurate observation, to say that we are to trust to spontaneous contractions of divided arteries ; or to clots blocking up the canal of an artery ; or finally and permanently mending an aperture in the side of a punctured vessel, would

be unproductive of serious consequences ; yet to minds untrained in the school of practice, those reveries of the closet serve as most fatal lessons. Upon the first authority, I can assure you that we are not without example in this city of the application of this doctrine of spontaneous contraction and natural coagulation, to the great disappointment of the surgeon, and fatal result to the patient. Indeed, to me this return to the obsolete doctrines of Petit and Kirkland, is little better than the revival of the old affair of the wonders wrought by stypticks, &c. upon the altar of which so many thousand lives have been immolated. But that I may not appear to wish to preoccupy your mind in behalf of what I am about to say on the experiments of the Doctor, I will present them, in their prominent and most characteristick features, to you, along with my remarks.

To be particular, which I intend to be, I must unavoidably be a little prolix ; too much so for a letter ; but the importance of the subject must serve as my apology.

Préparatory to the discussion of the main points more immediately the subjects of consideration, it may not be amiss to premise a few observations, on the structure of arteries, and also on their general function, viz. their process in receiving and distributing the blood. On this general function, the Doctor has been rather silent.

“ The substance of which arteries are composed is divided into distinct parts,” says the Doctor, “ which have been called tunicks or coats. Three coats, which have received various names, can be readily demonstrated, and may be simply and clearly distinguished by the terms, internal, middle, and external.

“ The internal coat, although extremely thin, is very close in its texture, and gives to an artery a smooth and polished lining ; it is elastick and firm, considering its delicate structure, in the longitudinal direction, but so weak in the circular as to be very easily torn by the slightest force applied in that direction. The morbid changes which have been observed in it, prove that this coat is vascular, and some experiments have been related to show the probability of its being sensible.

“ The middle coat, which is thickest, is formed by numerous layers of firm, compact, fleshy fibres, of a pale red colour, passing in a circular direction, but appearing rather obliquely connected and interlain with each other, than forming complete circles. These fibres are of a peculiar nature, are well supplied with nerves, and resemble, in form and disposition, muscular fibres, but differ from them in possessing a remarkable degree of elasticity. Their elasticity keeps a dead artery open and circular ; for this coat, when detached from the internal and ex-

ternal coats, still preserves the cylindrical form, whilst they, on the contrary, in a state of separation, become flaccid, and collapse. As this coat has no longitudinal fibres, the circular fibres are held together by a slender connection, which yields readily to any force applied in the circumference of the artery. The middle coat is intimately connected with the internal and external by a very short and fine cellular membrane.

“ The external coat, anatomically considered, is so simple, that many authors have thought it sufficient to say, that it is formed of condensed cellular membrane, which, by becoming gradually of a looser texture, connects the artery with the surrounding parts ; but the importance which is attached, in a surgical view, to this coat, renders a more particular account of it highly necessary and interesting. Although ultimately resolvable into cellular membrane, yet it derives from the particular arrangement of its component fibres, a characteristick appearance, which distinguishes it from cellular membrane, and entitles it to be ranked as a proper coat of an artery. Internally, or next to the middle coat, its texture is close and smooth ; externally, more open and rough, in consequence of the cellular membrane by which it is connected with an additional covering. The whole is remarkable for its whiteness, density, and great elasticity. *If an artery be surrounded by a ligature, its middle and internal coats will be as completely divided by*

it as they can be by a knife, whilst the external coat remains entire ; a fact which will be commented upon in another part of this treatise, and shown to be connected with important circumstances. The strength, therefore, of any artery depends chiefly on its external coat, which answers in some respects, the purpose of a strong fascia."

The above quotation I consider to be necessary, not because you and I could not recollect what anatomists have advanced on the structure of arteries, or what we ourselves have demonstrated it to be, but because, being in language peculiar to, and admitted in doctrine by Dr. Jones, I can the more easily show that his physiological and pathological observations are not always on the best terms with each other.

According to its structure, as above developed, if an artery be wounded transversely, the edges of the wound recede to a certain distance from each other ; if it be wounded longitudinally, the lips of the incision in the same manner, for the fibres of the middle coat are nearly circular, retire from each other, leaving a longitudinal chasm ; and if an artery be completely divided in its transverse direction, the extremities retract, the one towards the breast, the other from it, to a considerable distance ; but the diameter continues the same, for the "elasticity of the fibres, circular in their direction,

of the middle coat," we are assured by Dr. Jones, "keeps a dead artery open and circular."

In page 114, Dr. Jones, detailing the immediate effects of wounds of arteries, as they respect the form and appearance of such wounds, tells us that "the longitudinal appears to produce the slightest possible, or perhaps scarcely any separation; the oblique occasions a separation proportioned to its extent; and the transverse, however small, seems to produce a circular aperture in the parietes of the artery." And in note C, refers us to an opinion of John Bell, expressed on a case of Deschamp, which decides a principle.

Have the circular fibres of the middle coat no power or action? When the circle is violated by the slit-like cut up and down the wall of the artery, what prevents the fibres of the middle coat from acting? Will they not retract and gather up on themselves? They have the same elasticity that the longitudinal fibres have. I am astonished at the facility with which Bell has suffered himself to be led astray. With Dr. Jones there is more ample apology; he is evidently, from his writings, not a practical surgeon. Do we not see, too frequently, in unhappy operations with the lancet in the common affair of abstracting blood from the arm, that in those slit-like wounds the lips do not remain in complete contact and apposition? In operating

on the basilick vein, if we penetrate so deep as to touch the artery, we for the most part make the incision pretty nearly parallel with the longitudinal fibres of the humeral artery, descending through the fold of the elbow, just before it divides into the ulnar and radial trunks. And yet who ever understood that such wounds would not open in obedience to the elastic power of the circular fibres of the middle coat? Is there no separation of the lips in those longitudinal cuts, when a new wave of blood is impelled along, forcing the artery into a state of diastole? Surely, my friend, hypothesis has some share in this grave doctrine.

But to put all conjecture to rest, I have seen, and demonstrated, in a living artery, a longitudinal incision to the extent at least of half an inch: and in this the edges receded very considerably from each other, and exposed, in a great degree, the opposite internal surface, although the vessel was kept in an extended state, thus mechanically favouring the approximation of the edges, and counteracting the natural retraction of the circular fibres. Six or eight medical gentlemen witnessed this.

Were I not writing to Professor Smith, I would take into account, in my behalf, the opinion of Monro the father, in his great work, where he says, that if an artery be but slightly wounded, the edges instantly retract, and have

an aperture as if an artist's punch had passed through.

But this error, as to the state of the lips of a longitudinal cut in the parietes of an artery, so directly in the teeth of the anatomy and obvious physiology of the artery, is not the only thing of this quotation I have to notice. I have also to call your attention to the effects of a ligature thrown round an artery. The Doctor says, that "if an artery be surrounded by a ligature, its middle and internal coats will be as completely divided by it as they can be by a knife, whilst the external coat remains entire." If this position, which the Doctor has dignified with the rank of a fact, is intended to convey to the mind of the reader the idea that this division of the internal and middle coats is the usual consequence of a ligature applied in the ordinary way by a surgeon, and it can have no other surgical import, I am far from admitting it. It is, in my view, altogether gratuitous, and not deducible from any data furnished by the Doctor's experiments. But I shall postpone all further consideration of this until I come to the experiments instituted for its illustration. I now advance to the experiments of the first chapter.

CHAPTER I.—SECTION II.

Experiment 1.—"The hæmorrhage did not cease (from the carotid of a horse) before its

death. *The circular membrane surrounding the divided portions of artery was very much filled with blood; both extremities of the artery were considerably contracted, particularly the one next to the head, but there was no coagulum," &c.*

Experiment 2.—"The animal bled to death, &c. the truncated extremities of the artery had retracted about an inch and a quarter, and were very much contracted, but their mouths were open; *the arterial sheath and cellular membrane were much distended with blood," &c.*

Experiment 3.—The cut extremities of the artery were separated nearly an inch; both appeared to be *very much contracted; the surrounding cellular membrane and sheath were very much filled with blood," &c.*

It were a waste of time to recapitulate in detail, all the particulars of the nineteen experiments of this second section. It may suffice to say that in each one, the above prominent facts were fully ascertained, viz. *the contraction of the mouth of the divided extremity, and injection of the cellular membrane, surrounding the artery and sheath, with coagulated blood and lymph, and the mutual recession of the ends of the separated vessel.*

There can be no dispute about the retraction of the extremities of a divided artery, therefore I shall pass on to the other points.

By every stroke of the heart a wave of blood is urged forward into the artery, by which the artery is dilated, or is forced into what is termed its diastole ; the elasticity of the tunicks of the artery being by this wave of blood called into operation, for the coats are put into a state of distention, restores the artery from this state of dilatation to its ordinary quiescent state, or condition of systole ; and in this condition of rest or systole it would continue, were it not again altered by a succeeding wave of blood from the heart. In this play of systole and diastole consists the pulsation.

It is necessary here not to mistake this systole of the artery, this natural quiescent state, for what the Doctor calls the *contraction* ; for if the artery were dead and dissected out of the body, and laid by itself on a table, it would retain this systolick condition. Elasticity is equally the property of living or dead matter. The Doctor wishes to convey a far different idea to the mind of his reader ; he is desirous to impress him with the persuasion that the artery acted on by some unexplained laws, or spontaneous causes, is, by way of provision against approaching ruin, able to contract, and really does contract the mouth of the severed extremity.

The experiments uniformly present to us two points ; the one is the reduction of the diameter of the end of the artery, or what Dr. Jones terms

the *contraction*; the other is the infarction or injection of the cellular substance surrounding the artery. Now if this infarction or injection of the circumjacent cellular substance is fully adequate to the explanation of the reduction of the diameter, or lumen of the artery. why should we be overtenacious of this *contraction*? why should we so solicitously lookout for the support and maintenance of this child of the imagination?

When the arterial sheath is injected with blood, and the surrounding cellular membrane much filled with lymph, as the most pointed testimony of each experiment unequivocally establishes, either this cellular membrane surrounding the vessel must be enlarged, forcing apart and displacing all the superincumbent fat and muscle, and causing the volume of the limb, if it be in an extremity, to be increased; or the parietes of the artery must be urged into smaller compass, viz. the diameter of the artery must be lessened. The arterial tube is empty, or if filled, it can only contain coagulated blood, a material soft, and yielding, and readily giving place to a force from without.

Can rational, enlightened minds hesitate a moment about this alternative? It is not more plain that two and two amount to four in common arithmetic, than that if there be an internal force of one thousand, operating against a

counteracting force of five, the internal force still accumulating, that the counteracting force of five must give way.

From the nearest and best view I can take of the subject, I am convinced that the cellular membrane surrounding the artery is highly injected; that the end of the cut artery is reduced in its diameter; but this is done mechanically, and not by any spontaneous contraction in the vessel itself; nor do I see any insurmountable impediment to this conclusion, except it be the very respectable authority of Dr. Jones, who, with all the serious gravity imaginable, assures us, in the eighteenth experiment, that "there was a considerable layer of coagulated lymph, in which the artery was completely enclosed, but *not compressed* by it, although it was impossible to conceive a more complete engorgement of the cellular membranes surrounding it." And, to enforce with additional energy the doctrine of the text, our author adds, in a foot note, "I make this observation because the state of this artery forms an insuperable objection to Pouteau's doctrine, that the suppression of hæmorrhage depends on the engorgements of the cellular membrane." Then the Doctor does verily believe that, "although the portion of artery next to the head was so completely surrounded by coagulated lymph, that it was impossible, from an external view of it, to say exactly where it terminated," the whole force of this ac-

cumulated and accumulating lymph spent itself in displacing the superincumbent fat and muscles, and even putting the integuments themselves on the stretch, without at all compressing the parietes of the artery! Had Pouteau laboured seven years he could not have hunted up an experiment more to his purpose. And to what state of the artery Jones can possibly allude when he says it forms an insuperable objection to Pouteau's doctrine, I cannot conjecture. If the subject were not the adjustment of an important point of science, I should believe the Doctor was sporting with the easy faith of his reader.

The conical state of the extremity of the divided artery we also deduce from this coagulated blood and lymph compressing the sides. Why it is conical is very obvious; the coagulation immediately at the point is greater than it is higher, consequently the extreme end is more diminished in its volume than farther on.

In section the third of the same chapter, Dr. Jones proceeds to show that we are not only to expect, in the natural process for the suppression of arterial hæmorrhage, a contraction of the ends of the vessel; but, in addition to this spontaneous contraction, that we are uniformly to meet with an external and internal coagulum of blood, together with a coagulum of lymph interfused between those sanguineous coagula.

No surgeon of any reading or observation can hesitate to admit the existence of an *external* coagulum of blood; it is met with on every examination, and in every surgical writer. We cannot open a fresh wound without seeing it. The evidences however, of the uniform occurrence of an internal coagulum are not so great, and so generally within the reach of our senses, nor of that genuine, irrefragable sort which are to be collected from the faithful history of unadulterated cases. The coagulated lymph, on which the Doctor lays so much stress, is certainly to be found, but when he undertakes to show that it serves as a coagulum, or stopple, to close up the tube of the artery, he undoubtedly becomes too mechanical in his idea; but more of this presently.

In conceding the presence of an *external* coagulum in the sheath, and attached to the mouth of every recently divided artery, provided the artery be not of the larger class, I must not be understood as granting also its permanent utility. Its advantages are transient and illusory. By mechanically filling up the sheath left vacant by the retraction of the extremity of the vessel, and attaching itself, by means of the cohesive property of the coagulable lymph incorporated with it, to the end of the artery, it stops the hæmorrhage, but no sooner does this coagulum begin to liquify, and melt down, than this whole mechanical support gives way, and a new dash

of blood disquiets the patient, and alarms the surgeon.

I have seen this coagulation about the mouth of the vessel, this suppression of hæmorrhage, and this liquifaction and falling away of the mechanical transient support, again and again occurring, and it will constantly overtake the steps of the surgeon so long as he shall pursue the deviating track which keeps in sight the permanent aid of this external coagulum.

An *internal* coagulum of blood is not always to be met with; so rarely indeed does it occur, that many of the best and most experienced surgeons have searched for it in vain, for it has been a moot point nearly an hundred years. Nature in her processes, whether acting by the co-operation of many means, or simple causes, is, and must be uniform: a thing that is contingent is not of the regular process of nature.

White, in his cases, says, "I next laid open the arteries to their extremities, and found them entirely closed (sides completely in contact and consolidated) near an inch from the end of the stump; but from that point upwards *their capacities were not at all diminished*, nor was there any *coagulum or clot of blood* in the vessels or any where near them."

Pouteau remarks, that he "very seldom found any traces of internal coagulum; that in four ex-

amputations, one of a stump of an arm eight days after amputation; two of legs, one ten days, and the other three weeks after the operation; and of a thigh four weeks after amputation, he found no coagulum."

It is very extraordinary, if this internal coagulum be a part of the process of nature, that those two able and observing surgeons could discover nothing of it in those five or six cases. But Dr. Jones, anxious to break the force of these cases bearing so immediately on this disputable point, anticipates the conclusion of the reader, by reminding him that artificial means were resorted to. It is true they were; but will Dr. Jones, or any other gentleman for him, demonstrate that this artificial aid has prevented, or can prevent such a process, if it be a work of nature? I confess that in this suggestion of the Doctor, there is, in my opinion, more of evasion and subterfuge, than argument. What can be the difference between the subsequent operations of the extremity of an artery when a ligature or compress is used, and when this extremity is compressed by a coagulation round the parietes, thereby forcing them into contact? The external coagulum as readily forms in the artificial suppression as in the natural, and the effusion of the lymph is the same; for the effusion of lymph and the external coagulation are totally independent of the means, whether natural or artificial, made

use of for the suppression of the hæmorrhage. The effusion of the lymph is produced by the excitement in the exhalents, caused by the disturbance in their economy by the cutting instrument, and the coagulation is a mere property of dead matter, for a time acting in the coagulable lymph or gelatine: and the effusion of lymph is uniform, internally as well as externally, but it is not seen after a few days, because it is partly taken up by the absorbents of the internal coat, and partly carried away by the circulating fluid. But to proceed.

“ After he (a large horse, the subject of the experiment) was killed,” says Gooch, “ we dissected the thigh, and found that the bleeding was not suppressed by coagulated blood, but by the vessels being close contracted (their walls mechanically compressed) for near an inch or more from their extremities.”

“ When hæmorrhage stops of its own accord,” adds John Bell, “ it is neither from the retraction of an artery, nor the construction of the fibres, nor the formation of clots, (internal coagula of Petit) but by the cellular substance, which surrounds the artery, being injected with blood ” Here Bell repeats to us the doctrine of Pouteau, which is firmly established by the experiments of Dr. Jones.

On this opinion however, of John Bell, Dr. Jones indulges in the following criticism: “ We

have therefore only to discuss the difference between a *clot of blood* and *coagulated blood*, to discover the principal difference between this offspring of Mr. Bell, and what he has been pleased to call Petit's sickly child."

From respect to Dr. Jones I would not say that he has willingly misrepresented Bell, but most unquestionably he has egregiously misunderstood him. When Bell says, "nor the formation of clots," he, to the understanding of every man, plainly conveys the idea of the *internal* coagula of Petit, for against Petit's doctrine he is contending, and cannot be supposed to have in his view the *external* coagulation or injection of the surrounding cellular membrane, which is the true and real cause of the suppression. Dr. Jones, in his next edition will, I hope, leave both his candour and understanding unincumbered by suspicion.

That there is now and then an internal coagulum, I admit to be probable, yet I cannot grant it to be any part of the process of nature for the suppression of an arterial hæmorrhage; when it occurs, it can only be considered as a casualty, transient in its consequences, and of no permanent duration.

On the division of an artery, there is, at first, a dash of blood, but, if the artery be small, the effusion gradually lessens until the blood poured out shall coagulate, at first at a distance, and

by degrees it approaches the mouth of the vessel, and finally closes its tube by bringing the sides into contact. The irritation occasioned by the wounding instrument, after a few hours, causes the exhalents, both without and within the canal of the artery, to throw out an increased quantity of lymph; for the artery, in fact, inflames: the sides being compressed from without, are forced into contact and coaptated like the lips of a wound, a thin stratum or almost imperceptible quantity of lymph is effused between the parietes of the vessel, through which fine arteries, with all the apparatus of organization, pass, mutually receive each other, or anastomose, and ultimately constitute a continuous organized substance. Thus do the living walls, by these intermediate vessels, become permanently united; the blood circulates from side to side, and nature's grand object is attained. But yet, like an artist who, when he has completed his building takes down his scaffolding, nature, by apparatus of her own, gradually removes this effused lymph from the inside, from between the tunicks, and from without, and the whole artery, down to the coalesced points, is restored to its healthy natural appearance and condition. The effused lymph serves merely as an appropriate bed for the tender vessels to generate in, and no sooner is the organization accomplished than this now useless matter, is carried away by the industrious absorbents. Thus stood the fact in the arteries dissected by myself, referred

to in my former letter ; thus stood the fact in the cases of Pouteau ; and thus stood the fact with the accurate and observant White. Nor in my opinion will the careful dissector ever find it otherwise if he waits until the whole process of nature shall be gone through. As to internal columns of blood or lymph either becoming in and of themselves organized, sending their vessels out to meet those of the sides, and ultimately constituting with the sides an organized living part of the animal system, it is a flight of imagination against which I seriously and warmly object,

Whether the sides of an artery close by a single line, as was the case with my subject, or are united for half an inch or two inches, it matters not ; in either case the union depends on the sides being more or less pressed into contact, and kept so by external agents, or the cohesive property of the lymph, until they shall firmly grow together.

So far as respects partially divided or punctured arteries, I delivered my sentiments in my former letter, and in Dr. Jones I meet with nothing that alters in the slightest degree my opinion as you then received it. I am not conversant with the pathology of wounds in the arteries of the lower animals, but according to the success of the Doctor in brutes, the anatomy between their wounds and similar ones in the human kind

must be very different ; at least, if the testimonies of surgeons are to be received.

In the third chapter, Jones treats of the immediate effects of the ligature, used I suppose in the ordinary way, of surgeons. He herein endeavours to show the direct consequences of the ligature to be a total separation of the internal and middle coats of the vessel. Early in his work he announced as a fact this consequence of the ligature, and this chapter is pretty much devoted to illustrate it by apposite experiments. The whole that I can gather from the most attentive perusal of the experiments is, that, when a ligature was applied round the vessel, and in a short time thereafter removed, there was found an effusion of lymph, sometimes in the form of a transverse septum cutting the upper from the lower part.

These experiments, as they are detailed, are extremely inconclusive, and, to my mind, quite unsatisfactory. To them there are several things to object. We, in the first place, have to ascertain whether, in the application of the ligature to the artery of the brute, more force was not exerted than is ordinarily used by surgeons ; in the second place there was no demonstration that these coats were divided at all, it remains a thing of inference from the effusion of lymph ; and conceding, which we do not, that a separation of the fine vessels necessarily preceded the

effusion, it does not follow that the immediate effects of the ligature was the division of the internal and middle coats, for the ligature might mechanically so affect the susceptible constituent parts of these tunicks as to induce inflammation, gangrene, and sphacelus of the whole circle within the loop. Every surgeon knows that the circle within the loop uniformly dies and falls away ; but this is a secondary operation, and does not take place until after the sides, immediately above the edge of the mortifying circle, have united and become firm.

But, in the third place, my chief objection is, that when a ligature is applied, even but for a short time, the interruption to the circulation in the vasa vasorum of the internal and middle tunicks, and the bruising they receive, must excite more or less of irritation, which quickly runs on to inflammation. In this state of excitement, innumerable facts assure us, that the exhalents pour out a vast quantity of coagulable lymph. Nor is it requisite that the integrity of the fine vessels should be violated, either for the production of the lymph, or absolute union of the surfaces. We could instance the copious effusion of lymph, along the internal surface of the trachea, in cases of the croup ; also, in inflammations of the pleura, the numerous occurrences of a permanent union of the pleura costalis with the pleura pulmonalis ; and to these may be superadded the various attachments be-

tween the several duplicatures of the peritoneum in cases of abdominal inflammation.

The Doctor certainly would not argue that, because the lymph was effused, or because internal surfaces coalesce, there necessarily must be a division of vessels or an ulceration of surfaces.

I now close my long, and, I am apprehensive, uninteresting letter. I have endeavoured to be plain and intelligible in all its parts: I have objected to no opinion that I did not think seriously objectionable; and I have in no place quoted Dr. Jones's sentiments, so as, by dissociating them from the general text, to make them appear to a disadvantage. I hate disputation, and I abhor a war of words.

Baltimore, October 27th, 1809.

*A review of Mease's case of Aneurism, by J.
B. Davidge, M. D.*

MEDICAL cases, collected with assiduity, examined with intelligence, and promulgated with ingenuousness, are ample lessons of instruction, but when they are permitted to be vehicles of error, or channels affording facilities to undigested, hasty speculations, they call irresistibly on our attention, and rouse our vigilance; they should be scrutinized with rigour, yet with justice, and admitted with the extremest caution; it becomes a duty to control their influence, and repress their extravagance. Were it so that they would fall into the hands of men of science and observation only, we should give ourselves but little concern about them, but as they are intended to convey, doctrines and principles, to minds incompetent to determine on their nature, it would amount to culpable dereliction of the duty we owe both the publick and our profession to let them pass unnoticed. In an especial manner are we bound to subject such cases, as are here alluded to, to strict and critical analysis, where they come forth clothed with the authority of character, and enforced by the recommendation of that celebrity which already and deservedly possesses the publick confidence.

Had the above case, to be found in the sixth number of the Philosophical Journal and Review of New-York, from the pen of Dr. Mease, been given to the world unaccompanied by the *preface* and *conclusion* by Dr. Dorsey, it would have excited little interest and awakened less curiosity. There is so much of common character about it; so much of what every professional man, at all in the habit of using the lancet, must frequently meet with, that it would have called forth few other sentiments than those of gratitude for the benevolent intentions of the respectable author, and, perhaps, surprise at the minuteness of a detail without the force of conviction or ornament of variety.

Upon reading the prelude, on which the editor of the Journal in a foot-note, makes a judicious remark, we acknowledge we anticipated something curious and valuable; such as, if it did not illustrate what the writer wished to establish, would at least have afforded entertainment to the practitioner and information to the pupil; but, in this our expectation, we were disappointed. The first point, in the detail of the case, deserving of remark, is "The blood at first appeared *very dark*, for I had been in high health."

That the blood when it first flows from a vein, recently opened, should be *very dark*, may be attributed to almost any circumstance rather

than to *high health*. We presume the author means by very dark, *unusually* dark.

The blood may be very (unusually) dark from any disease which may so affect the lungs in their function as to prevent the blood from being properly oxygenated ; or from the air being cut off from the lungs by submersion, or a cord round the neck. Contrary to what is the import and meaning of the writer's language, the blood is never very, or in any eminent degree, dark, exclusively from high health. It is true it may be very dark when the arm has been bound up any considerable time before the vein be opened, although the persons have been in high health. And we are not a little surprised that a gentleman of the professional respectability of Dr. Mease, should have permitted an idea so unphilosophick to have escaped from his pen.

“ In about ten or twelve seconds it (the blood) changed to a florid complexion, and was discharged in jets.”

From this the writer wishes it to be inferred that the former quantity, the very dark, was from the vein, and the latter from the artery. And to fortify this suggestion still more, he is careful to mention that it was discharged in jets.

If a ligature be passed round the arm for a short time and then the vein be opened, the blood first discharged will be dark in proportion

to the time the ligature was on previous to the opening of the vein ; if it be permitted to flow for a minute or two it becomes of a colour more or less florid ; and if the vein opened be the basilick, running immediately over the artery, the blood will be discharged in jets. Nor will those circumstances occur casually ; they are almost always uniform.

The blood is dark because it is detained in the veins of the arm, as it were out of the general circulation ; it becomes of a florid complexion because there are fresh supplies round from the arteries ; and it is discharged in jets because the great artery in the fold of the arm is strongly beating under the lower wall of the basilick vein. There is nothing in all this which can *decisively* point out the artery to be wounded, or should have led a practical man to have suspected such an accident-

“ I felt the pulsation of the blood, while flowing from the artery into the vein.”

Of this, no doubt, the Doctor felt satisfied, but he will excuse us, we hope, when we acknowledge that we can have no satisfactory conception how, or in what way, he could determine whether the pulsation he felt were of the blood flowing from the artery into the vein, or from the blood flowing along the channel of the artery, beating against the blood effused from the vein. We may repeat, that to determine

the pulsation felt to have been from the blood flowing from the artery into the vein, required more nicety and accuracy of touch, than in our estimation could have been boasted by any man, how well soever trained his sensation might have been ; and we are still disposed to propound the question : Is the pulsation of the blood while flowing from the artery into the vein obviously different from the pulsation of the blood while flowing in the tube of the artery ? and, If it be not indicated by some marked variety, might not the Doctor have been imposed on ?

“ The *pulsation* of the *blood*,” is an unusual expression. We understand the *pulsation* of an *artery*. Was there also a tube. possessed of the properties of an artery, between the vein and artery ? If not there could not have been a pulsation produced by the passage of the blood, upon the supposition of a communication. .

“ And perceived the thrilling noise from the same cause whenever I put my head under the bed-cloathes.”

How strongly soever the Doctor was persuaded that he perceived a thrilling noise whenever he put his head under the bed-clothes, we must be indulged in the opinion that there was more of *imagination* than *reality* in the perception. Fancy is sometimes extremely unruly, and at no time more so, than when a man had become

persuaded that the main artery of the arm was wounded. Particularly when the subject of this supposed injury is a physician, well acquainted with the consequences of such a wound.

Why could not the thrilling noise be heard except when the Doctor was in bed? Is *darkness* favourable to sound? It is to the exercise of the imagination. Or are vibrations propagated better under the bed-clothes than in the open air of the room? It does not appear that this thrilling noise was heard by either Dr. Physic or Dr. Dorsey.

“ I was careful to use my arm as little as possible, for besides the continuance there was a small swelling about the size of a small filbert in the injured part; but about five or six weeks after the accident I imprudently exercised it for half an hour, when the part swelled to the size of a common filbert, became painful and looked red. The pain and redness however soon subsided, but the swelling did not diminish for upwards of two months.”

Now we have before us the plain and unadorned fact; the simple description of an ecchymosis compounded with an inflamed and thickened tendon; such as we have all met with so often that we have almost ceased to notice them.

In the genuine varicose aneurism, the blood passes freely and uninterruptedly from the arte-

ry into the vein, especially so when a thrilling noise can be heard. If then there be a liberal and ready passage of the blood from the artery into the vein, and afterwards an easy progress on through the vein to the heart, how could an increase of swelling, a redness and pain occur? This pain, this redness, this increase of swelling, argues very strongly, to our understanding, that the case was an ecchymosis combined with inflammation and thickening of the circumjacent substance. Indeed except there be a prompt and ready passage of the blood from the artery into the vein, the vein can never become varicose; and if there be this ready passage of the blood we cannot understand how, at the distance of six weeks after the injury occurred, inflammation could take place; but that it did take place is obvious from the phenomena of redness, pain, and swelling.

“ But, says the Doctor, the swelling did not diminish for upwards of two months.”

If the case were a genuine case of varicose aneurism, what could cause the *swelling* to diminish at all, until the aperture in the artery were closed? The vein being once varicose, would continue to be so, and, indeed, increase in volume, so long as the blood should have free access to it from the artery.

“ Doctor Physick having carefully examined the arm, was perfectly convinced of the existence

of varicose aneurism, and did not entertain the faintest hope of a spontaneous cure."

The opinions of Dr Physick are respectable, and we are at all times gratified in paying regard to them ; but we are desirous it should be understood that the opinions of that gentleman, like those of every other gentleman of science and observation, must be considered as opinions, *mere opinions*, and can by no means be substituted for facts. And we are the more unwilling, in the present instance, to admit the substitution of *opinions* for *facts*, as we are informed that the able Physician referred to had not the faintest hope of a spontaneous cure. That is, neither his reading or experience had ever furnished him with a spontaneous cure of a varicose aneurism, and conceiving this to be one, he had no ground on which he could bottom his hope.

If neither the fair pages of surgical history, nor the honest experience of any judicious Surgeon, can furnish a solitary case of a spontaneous cure of a varicose aneurism, or indeed any other, without the obliteration of the diameter of the artery wounded, ought there not to be some little hesitation in making up a decisive opinion in the present case? But even granting that the artery was wounded, that it was an unequivocal aneurism, what evidence has Dr. Dorsey, from which he can reasonably or justly con-

clude that "the artery ever since has been pervious."

"At this time and ever since the cure, the artery has been pervious, and pulsates like that in the other arm at the elbow and wrist."

In this concluding sentence Dr. Dorsey indulges himself in an unreserved expression of a positive opinion on a subject, respecting which he could have had no decisive or positive knowledge. We will grant however to show more plainly the inconsiderateness of the Doctor's conclusion, that the artery was wounded, and that there has been a spontaneous cure. But does it follow as a necessary consequence from a spontaneous cure that the artery must be pervious? There are numerous instances of spontaneous cures of aneurism on record, but in all there was an obliteration of the cavity of the artery. On the contrary there is no case where the wound in the wall of the artery has been closed by accident or by nature, leaving the artery *pervious*.

The lumen of an artery may be obliterated to a certain extent and yet, in consequence of the touch by the finger, being confused by the pulsation of the artery above, and below the point of union or consolidation of the sides of the artery, no satisfactory or unexceptionable deduction can be established.

With regard to the pulsation at the wrist, on which some stress is laid, we hope it is unnecessary for us to suggest, to the learned gentleman, that the perviousness of the main artery at the fold of the arm is by no means necessary. He well knows, as every surgeon must know, that an artery pulsates, when supplied by anastomosing branches, its trunk being tied or cut asunder, as regularly and nearly or quite as forcibly, as when its diameter is free and open throughout.

It appears to be forgotten that there was, and that too for a considerable time a compress on the tumour. What prevented the sides of the artery from being forced into contact by a compress continued for a series of days? And if forced into contact, what prevented a union?

But Dr. Dorsey decisively and unhesitatingly avers that the artery is pervious. And by what sensible means has he ascertained this? we know of but one mean, unequivocal and satisfactory, by which this point can be settled, that is *injection* and *dissection*.

Surely gentlemen must be very solicitous to sustain the notion that wounds in arteries can be cured spontaneously without a closing of their cavities, and that the surgeon need not trouble himself about the ligature or compress which we declare to be *necessary* in all wounded arteries.

We will conclude the present remarks, with hinting to these gentlemen, and those who may fall in with their cases, that, if they do not obliterate the diameters of wounded arteries by compress or ligature, the fate of their patients will remain as sad memorials of the imperfect condition of the surgical art; and we will subjoin that we exceedingly regret, because we highly respect Dr. Dorsey, that, without *positive* proof of any kind, or *unequivocal* evidence of any sort, or even *probability*, he has permitted himself to publish the following paragraph: he should have recollected that a sufficient number of lives have already been lost from wounded arteries left to “heal without any obliteration of their cavity.”

“In compliance with my promise, I have the pleasure to subjoin the case of aneurism which I consider a *decisive* proof, that *wounded arteries may heal without an obliteration of their cavity*. I fully concur with you in the opinion that such a termination is extremely rare; but those writers who pronounce the thing impossible, are impossibly, are surely mistaken”!!

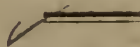
FINIS.





PHYSICAL
SKETCHES;
OR,
OUTLINES OF CORRECTIVES,

APPLIED TO
CERTAIN MODERN ERROURS IN PHYSICK.


BY

JOHN B. DAVIDGE, A. M. M. D.
Professor of Anatomy in the University of Maryland.

Quantula sunt hominum opuscula examen solum fatetur.

VOL. II.

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1816.

District of Maryland, ss.

BE it remembered that on this Thirteenth day of December,
in the Thirty-Eighth year of the Independence of
United States of America, John B. Davidge, of the
the said district, hath deposited in this Office the
Title of a Book, the right whereof he claims as Au-
thor ; in the words and figures following, to wit—

“ Physical Sketches, or outlines of correctives, applied to
certain modern Errours in Physick ; by John B. Davidge, A.
M. M. D. Professor of Anatomy in the University of Maryland.
Quantula sunt hominum opuscula examem solum fatetur.”

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in mentioned,” and extending the benefits thereof to the arts
of Designing, Engraving, and Etching, Historical, and other
Prints.

PHILIP MOORE,
Clerk of the District of Maryland.

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PREFACE.

WHEN an author comes before the publick, it has been remarked by a writer,* perhaps the most distinguished in polite and moral literature as yet furnished by society, that he should recommend himself by something new, either in form or substance ; something amusing or instructing ; variously ornamental or substantially valuable. This remark is founded in good sense and extensive propriety ; for a writer is not privileged to call on the time—the attention—or the patience of society, without offering some remuneration in the way of pleasure or instruction.

In matters of science, it is seldom that an author can successfully atone, by a richness and variety of diction, or gracefulness of style, for the contribution under which he necessarily lays the publick attention : and it can scarcely be expected that a writer, although he were copiously and extensively provided with the materials of the subject, could fur-

* S. Johnson.

nish much that is substantially new to professional men, on a point of a practical art so long and so frequently enquired into, as amputation. It is therefore hoped, that the author will be understood as chiefly addressing himself to learners, in the following pages—to those whose footsteps are entering on the threshold of their profession. To such, he is persuaded, a chronological history of an operation, so important and valuable as amputation, may be addressed with some prospect of advantage ; an operation which, although of primary consideration to the Surgeon and the community at large, has been more slowly progressive towards a decent and tolerable state of perfection, than the pride and vanity of our profession may be willing to admit. For, the author with becoming deference assumes it, since the days of Celsus, the Roman medical historian, to within about fifty years of the present era, there has been scarcely a change in the manner of performing amputation, which, in the opinion of a man of knowledge and observation, can be considered to be an improvement.

For the accommodation of the student, a regular graduated historical sketch, bearing relation to the earliest period at which this important operation first invited the attention of the profession, will be presented, sustained by suitable references and appropriate quotations, so that the reader will be enabled to detect

the impositions of the Plagiary, and ascertain where honest merit and enlightened claim have been despoiled, by ungenerous suggestion or ignorant interpretation.

And inasmuch as inflammation, under various modifications, so frequently involves the consideration of the practitioner, and as it is by no means remote, in some of its relations, to the present subject, the author deems it not impertinent to offer a few remarks on its principles.

The pathology of divided arteries is so intimately connected with amputation, that a work purporting to treat expressly of the latter, would, in a marked degree, be viewed as deficient, were it to leave unexamined the former; a suggestion or two will be laid before the reader, tending to weaken his confidence in the gratuitous opinion, advocated with more pertinacity than intelligence, that cut arteries spontaneously contract at their truncated ends. Not only does the author attempt to invalidate the hypothesis, that arteries at their cut ends contract, but he goes so far as to undertake a demonstration, founded on the most simple deductions from facts, in his apprehension incontrovertible, that if there be a change in the circle at the divided end, except such as arises from extraneous agency, it must be rather of dilatation than contraction.

To the main object of the present work the author has annexed a short, and, he is apprehensive, but imperfect Sketch of the Phenomena of Animation. This limited Sketch, is the first of a series which the author has had it in contemplation for some time to put together as an introduction to a general System of Physiology. Having laid down his general principles, he intends, at some period not remote, to enter into an enquiry on the Whereness of Life, whether its more immediate local relation be to the blood—the muscle—or the nerve, and then to offer some illustration of his doctrine, in a detailed examination of the individual physiological functions of the animal economy.

A TRACT ON AMPUTATION.

SECTION I.

Id ante omnia scire convenit, quod omnes medicinæ partes, ita connexæ sunt, ut ex toto separari non possint. *Celsus.*

AMONG all enlightened and civilized societies, surgery holds a rank, distinguished by its importance, and valuable from its practical utility. Connected as it is with human affairs through the mean of the evils and casualties to which life is subject, in the various and complex occupations and employments of man, it has ever been, in its cultivation, an object of primary consideration. In the very contexture of the surgical art the tenderest affinities of social endearments are interwoven, and to its improvement and success most closely allied ; for at no moment of time can the comforts or felicities of humanity be conceived to be disso-

ciated from its condition. Surgery is coeval with our kind, and admonished by our necessities, we repose ourselves on its support and resources. Thus from the close alliance maintained between the surgical art and the necessities of the human family, an imperious duty is imposed on the cultivators of this highly useful department of knowledge to lay open to the learner every avenue, and make accessible every source which invites to inquiry, excites to industry, or animates to professional acquisition.

In the general, our professional education is imperfect, and it is so, because a too limited view is, for the most part, taken of the profession. It is too much the fashion to consider it as the mere instrument, managed by intrigue or skill, of pecuniary emolument. When thus contemplated, most obviously, it is without the allurements of charm, or force of incentive. To the philosopher it is a region dreary and waste, and to the professional man destitute of honour and equivocal in gain. Under this aspect, unheard would be its calls on the generous affections of the heart, and the more animated efforts of ambition—that noble source of human action. But when viewed in the extended consequences which connect it with the dearest interests of society, and in the varied relations which associate it with the most sacred hopes of private expectation, or public prosperity ; the pretensions of the

healing art are more than realized in our expectations and fears.

It is, when viewed in such extension of its consequences, that the medical profession becomes affluent in the production of the characters and high qualifications which endear it to humanity.

The medical student is to be apprized that the first step he is to make is, that of becoming acquainted with the value and importance of his profession ; and having learned that his profession has its origin in the doubtful felicities and calamities of the human family, he is, by a sober and deliberate examination, to make himself conversant with the various authorities that have poured in their aid and their active services during the periods the art has been cultivated. The whole vista, extending into times that are past, is to be opened, and each author in his own claims, and every invention in its proper attitude, brought into view.

Where there is not high talent, there may be industry, in the student ; and he who cannot unfold the rich stores of genius on the stage of active exertion, may be admonished not to disappoint our anticipation by an empty understanding. Elevated intellect is of the rich munificence of heaven, but, for the encouragement of manly enterprize, knowledge

- may be acquired by sedulous care, and vigilant attention. With mind we must be endowed, but improvement is of labour.

To adduce to view points of science and expertly revolve them on the pivot of a proposition, is a slender part of his duty, who has undertaken to prepare young gentlemen for the discharge of the duties of an arduous, honourable, and responsible profession. What will avail the graceful address of a flexible ingenuity ; the overwhelming force of well disposed argument ; or the persuasive, various, and captivating flow of a copious eloquence, to a youth unread in his profession ; to ears unfamiliar to sounds of their own art ? To minds thus unprepared, by circumspect, historical training, dexterously to appropriate the rich treasure, the language of the science, the very words in which attempts are made to convey knowledge, would be strange and incomprehensible. The learner hears or reads, and is amused ; he admires and is ignorant. Without materials to work up, genius itself would be idle ; fancy might conceive, and decorate the sickly offspring of its own fertility, but where would be the energy and the robustness of frame to ensure longevity ?

Without a knowledge of the history, every man would be rather an animal of instinct or a savage in relation to his profession than a member of an improved and civilized commu-

nity. His whole professional learning would be the vacillating structure of individual observation. Without connection with what is past, or a relation to what is to come, the whole sum of his acquirements would luxuriate into extravagance, or, in unfinished forms, dissolve away like the perishable dream of an infant.

But when provided with the professional opulence of ages the learner may retrace, examine, and compare ; he feels himself rich in the stores piled up by centuries. His labours are productive because they are prudently directed. Fields already found to be unfertile are not again to be cultivated, and quarries once exhausted, no longer invite the hand of industry. He who conceives science to be the work of a day overrates human powers.

In pursuance of this apprehension of the manner in which a science is to be acquired I shall proceed to address myself to the attention of such as may favour these pages with a perusal, and exhibit, in detailed order, the various methods of amputation, so far as interesting varieties may entitle them to notice, which have been in use from the earliest periods of surgery to the present time ; filling up the spaces interjacent to the precedent and subsequent authorities with such remarks and reflections as may be naturally derivable from the writer's manner of treating his subject.

And thus offer to the reader's contemplation a general plan, the various parts of which are reciprocally conjoined by materials spontaneously furnished from their proper texture. I shall trace out to his view, in a simple and comprehensible form, the whole course of amputation.

It may not be amiss at the present stage of my undertaking distinctly to advertise the reader that operations at the joints will constitute no part of my scheme. Operations at the joints, at least larger joints, but seldom occur, and when they are considered proper, some peculiarity of structure in the part or leading character in the disease, demanding such procedure, will dictate rules to the manner. Flaps, however, varied as they may be, must always come into the constitution of the plan. Nor will it be improper to mention that I shall say nothing as to the general question of the propriety or impropriety of amputation, under supposable circumstances, so much and so keenly contested by some writers and practitioners of Europe. The utility and necessity of this operation are so decidedly the subject of fact; a plain matter of common sense and common experience, that it were affronting to the reader to offer an argument on the affair.

The opposition it has experienced from a few men of professional eminence of continen-

tal Europe, is to me rather a melancholy proof of the depressed condition of the surgical art in that division of the world than what a man of sober sense would attempt to urge against the practice itself.

If the reader wishes to look into this subject he may, at his leisure, consult Bilquer and the justly celebrated Pott.

Amputation, like every other operation in surgery, is various in the objects to which it bears relation. Its provisions are the prevention of evil, and the production of benefit. If a member be so impaired or seriously injured as by its relation to endanger the safety of the body, immediately or remotely, it falls under the province of amputation to provide security to the whole by a removal of the part. And this in a way that will lay the general economy under as little contribution as may be. Not only is the pain to which the patient, during the operation, is of necessity subjected ; the time consumed in the manual execution ; the management of the blood-vessels, the care of amputation, but all inconveniences consequential on the process, whether they arise from any undue length of time in healing the wound ; the protrusion of the bone ; its exfoliation ; or deformity and unseemliness, for surgery should be neat as well as secure, are equally within the scheme contemplated by amputation. In short it is the province of

this important operation to remedy ill, and produce good, to save from incommmodity, temporary or permanent, and to restore to social utility and personal ease and comfort, so far as is possible.

Having proceeded so far in the evolution of the principles and explanation of my purposes, I shall now lay before the reader my authorities and grounds of procedure.

CELSUS. } “ ‘The flesh (the integuments and muscles) must be divided to the bone, the knife being directed between the healthy and the gangrenous portions in such a way as rather to take from the healthy than leave any part of the diseased. Care is always to be taken to avoid a joint. When the surgeon has arrived at the bone the flesh (the integuments and muscles) is to be retracted, it being previously detached from the bone, so as to expose a portion of the bone, which is to be divided as close up to the healthy soft parts as possible. The end of the bone is then to be smoothed off, and the integuments to be laid as completely as they will allow, over it ; but they should be loose and easy. On that part of the face of the stump not covered by the skin is to be laid a dressing of mild ointment, which is to be secured by a sponge charged with vinegar. The subsequent measures are to be ordered as is customary in wounds in which it is the object of the surgeon

to heal by the first intention; or in which *pus* ought not to be promoted."*

It is generally admitted that Celsus, who lived in the time of Tiberius, the third Emperor of Rome, was rather a medical philosopher and historian, than practically a physician. In his elegant and elaborate medical records we are to discover the dominant opinions and practice which had currency among the Roman people, and not his private sentiments only. Antecedent to the period at which he wrote, there is little or nothing to be met with in anywise clear or definite on amputation. For although Hippocrates and others treated of gangrene and sphacelus, and promulgated opinions on the propriety and necessity of removing that which was mortified and dead, yet they are altogether silent as to the *manner* of conducting the thing. Hippocrates, however, as may be seen in the latin version to which I have had access, is very emphatical. The remarkable passage is as follows :

* " Igitur inter sanam vitiamque partem incidenda scalpello caro usque ad os est sic, ut neque contra ipsum articulum id fiat, et potius ex sana parte aliquid excidatur quam, ex ægra relinquantur. Ubi ad os ventum est reducenda ab eo sana caro, et circa os subsecanda est, ut ea quoque parte aliquid ossis nudetur : dein id serrula præcidendum est quam proxime sanæ carni eatiam inhaerenti, ac tum frons ossis, quam serrula exasperavit, lævanda est, supraque inducenda cutis, quæ sub ejusmodi curatione laxa esse debet, ut quam maxime undique os contegat. Quo cutis inducta non fuerit, id linamentis erit contegendum, et supra id spongia ex aceto deliganda. Cætera postea sic facienda, ut in vulneribus, *in quibus pus non moveri debet, præceptum est.*"

Celsus, p. 498.

“ Illi affectus qui medicamentis non sanantur, ferro sanantur ; qui ferro non sanantur ; igne sanantur ; qui igne non sanantur, hos existimare oportet insanabiles.”

Hippocratis omnia opera.

This is truly, both as to form and substance, the anceps remedium in ancipiti morbo—The uncertain remedy in the doubtful disease.

The inadequacy of the operation as detailed by Celsus, to provide for the various indications involved in its pretensions, is apparent to the most superficial observer. It is obvious, that by the very first circular incision through the skin and muscles down to the bone, the surgeon effectually deprives himself of every possible advantage resulting from a protection to the face of the divided muscles and bone derivable from the integuments. And the dissection of the muscles from round about the bone is, as to extent, culpably equivocal. The object to be accomplished is too important to be so indefinitely expressed, as in the words, “ so as to expose a portion of the bone.”

Yet we are struck with the manner in which this very able writer closes his account of the process. “ The subsequent measures are to be ordered as is customary in wounds in which it is the object of the surgeon to heal by the

first intention, or in which pus is *not* to be promoted." "In quibus pus *non* moveri debet."

The manner of the operation, however, puts an end to all expectations of this sort. But we infer very fairly and confidently, from the language, that it was the custom of his time to aim at a cure by the first intention under circumstances favourable to such a mode of managing wounds. Our profession was rapidly advancing among the enlightened Romans.—Indeed, the period, and other matters being considered, in regard to Celsus we have but little room for complaint ; great for commendation. But the dark ages, not long after succeeding, all science, and art, and improvement, were involved in profound obscurity ; hope failed, and zeal ceased to anticipate.

From the days of Celsus, through a long and dreary period, until the time in which Wiseman flourished, there is scarcely a ray of professional light to guide the wandering steps of research : We turn over the pages of this almost unbroken lapse of ages with unrewarded industry ; nor did science, when, after a night of about seventeen hundred years, it again dawned upon the world, discover much to cheer our despondence or establish our confidence ; our profession was in regression returning, in rapid strides, to the original confines of primæval barbarism.

So that, with Wiseman, we have to commence a new æra.

AMPUTATION.

WISEMAN, } “ But where amputation it-
1676. } self is feasible, nay altogether
necessary, as in the limbs, there
ariseth a new question, whether it ought to be
done in the sound part, or in the dead. Fab.
ab Aquapend. adviseth the dead part, within a
finger’s breadth of the live, (as also do many
others.) Then with a cautery they burn up
all the remaining mortification. Others do
perform the operation with a hot knife, the
figure of which you may see in Hildanus.—
Aquapendens speaks so confidently of his suc-
cess in the practice of this way, that, utterly
to deny the thing, were to give him the lie. I
shall only make this objection, that the sphac-
elus doth seldom affect the member so equally
round, as that an operation in the dead part is
feasible : Nay, which is worse, when once the
mortification seizes on the great vessels, it run-
neth up so fast under the skin, that before any
considerable part of the member is outwardly
gangrened on the one side, beyond the knee or
elbow, the other side will have reached to the
Inguen or Axilla. This, I confess, frustrates
all manner of amputation, whether in sound
or mortified parts. Yet, generally speaking,
the gangrene doth not grow so fast, but that,
if you make amputation two finger’s breadth,

more or less, within the live flesh, you may prevent it ; and that far better than it is possible to be done by the other way of operating. For if you work there by actual cautery, with a hot knife, your knife will stick fast in the flesh till the moisture be wholly dried up, and then you must repeat many knives and cauteries, in every one of which you will meet with the same inconvenience ; yea, before you can cut through with your hot knife, it is very probable that you will, near the bone, meet with some parts that are alive and sensible, especially when you come to the periosteum, (for the membranes do not corrupt at the same rate of swiftness with the flesh ;) and then all your pains and trouble will be as great, or greater, than if you did it in the sound part. As to the other inconveniences I shall refer you to Hildanus, who hath taken pains in confuting this opinion."

" Both these ways of amputation seem to me much more difficult and painful than our common way ; which makes me presume they proposed them to prevent the effusion of blood."

" But modern surgeons have answered this objection by good bandages and deligation of the vessels, and when we cut above the knee, by clapping cauteries to the vessels only ; which sufficiently answers that intention.— Thus have I seen, in my first sea-voyage, am-

putations made above knee, and afterwards practised them with great ease and safety."

"Now, since not only in this chapter of gangrene, but frequently elsewhere in this book we have mentioned amputation, I think it necessary to take this occasion of shewing the manner of performing it; the rather because the operation is much the same, whether it be done upon the account of gangrene, or for other reasons. I shall therefore, in general terms, insert the whole doctrine of amputation, though briefly, not mattering whether the examples instanced be sphaceli or not, all instances being alike instructive."

"In heat of fight, whether it be at sea or land, the chirurgeon ought to consider, at the first dressing, what possibility there is of preserving the wounded member; and accordingly, if there be no hopes of saving it, to make his amputation at that instant, whilst the patient is free of fever, &c. &c."

"The member is to be supported by some one, whilst another standeth behind the patient, and draweth up the skin and musculous flesh. Then make your ligature two fingers breadth, or thereabout, in the sound part; so that if you amputate in case of mortification, you may be sure to quit yourself of it. This ligature is omitted by many of our chirurgeons here in this city, they only making a turn with

a tape, pinning it on as a mark to circumcise by : and instead of the ligature, I propose, they make a gripe, which gripe is made by some assistant who hath strength to do it. Indeed this is good, where a shattered member is to be cut off, to smooth the stump ; in which case there is no great fear of bleeding. But in amputations it seems to me to be very inconvenient : for I yet never saw any man so gripe but that still the artery bled with a greater force than was allowable ; yea, when Mr. Woodal griped, who was so applauded, and, in truth, *made* for the work. It being so, in what a huddle is the stump then dressed. But suppose the uneasy posture and the long gripping tires the assistant, or that his hand be cramped the while, what condition is the patient then in ? Whereas, by this ancient way of ligature the vessels are secured from bleeding, the member benumbed, and the flesh held steady, ready to receive the impression of your crooked knife, or razor, which I have often amputated with."

" 'This ligature made, the assistant strengthens it, whilst he draws up the musculous flesh. In the mean time the operator with a sharp crooked knife, by a turn with his hand cuts the flesh off round to the bone ; then with the back of it he scrapes the periosteum from the bone ; if there be two bones, then with a dividing knife he separates the fleshy membrane from them. The bones freed of the

periosteum, Guido proposes a linen cloth, and Hildanus a kind of purse, to be brought over the upper divided flesh to pull it upward, and make more way for the saw. But I think that needless. The flesh divided, the parts separate enough of themselves, besides, the assistants pulling up the musculous flesh is sufficient. That done you are at liberty, whether you will cauterize the vessels by a button cautery, or by ligature stop the bleeding, or by agglutination. The use of *chalcanthein* I do not approve. To apply escaroticks to the end of the nerves and tendons newly incised, causes great pain, weakens the part, and makes way for gangrene ; it not being likely you can apply them so to the artery, but that you must burn the parts about, which are, as I said, the nerves, &c. The way Hildanus proposes, by drawing the vessels out by a forceps, is not a work to be done in the heat of fight, nor without a clear day-light, &c. Ambrose Parè proposes a more easy and sure way of deligation, by passing a needle with a strong twisted thread through the skin near the great vessels ; making your stitch over the said vessels by piercing through the raw flesh and skin : then make your ligature upon the fold of a rag. Thus you bind the artery and vein.—These several ways have been practised by eminent surgeons for the stopping the blood of the arteries in amputation : But the late discovery of the royal styptick hath rendered them of less use. But in heat of fight it will

be necessary to have your actual cautery always ready, for that will secure the bleeding arteries in a moment, and fortify the part against future putrefaction. They require after cauterization no such strict binding as that, thereby you need to fear interception of the spirits. When we cauterize the artery, we do then touch the end of the bone, it hastening the exfoliation. The next thing, the loosening the ligature, and bringing the lips close over the end of the stump. Then, whether you should with a cross stitch hold them so, or content yourself by bandage as well as you can, is by some controverted.— They that object against the former, say it causes pain and inflammation. So doth bandage also. The most that I have seen without the cross stitch, have, the next dressing, been broad stumps, some of them with lips turned outward by the bandage ; in the least of them the whole stump hath been bare : Whereas, in that in which I have made the cross stitch, the lips have been found close to the bone at the next dressing, covering the great vessels, and a third part, or half of the stump, hath been well digested, and, by the second dressing, hath been nearly agglutinated so far as it lay under that skin, and, without inflammation, happily cured. Whereas the broad stump is a certain sign of a long cure, and commonly the death of the patient. The exposing these incised parts too much to the air causes pain, and the want of a native heat makes them

gleet: or, if they do digest, the discharge of so great a quantity of matter commonly exhausts the spirits of the patient. If the musculous flesh and skin are well pulled up in time of amputation, and brought over by moderate extension as far as they will easily admit, you will find it not painful. You ought to pierce the skin with a needle and strong brown thread ceared about half an inch from the edges of the lips. Having made them cross from side to side equally, tie them with a moderate astriction, then apply a pledget on the cauterized vessels, with *pulv. iind. suc. cepar. cum melle*. But if no cautery was used, then buttons of tow spread with your restrictive, and dipt in *pulv. Galeni* must be applied to the mouths of the vessels. Afterwards sprinkle the stump with this powder: *Bol. Armen. farin. volatil. picis. naval. Aloes, thuris, mastich. Sang. dracon, &c. &c.* You may mix some of the same *cum album. ovor*, and apply it upon a thick round stupe, which hath first been wet in oxerate and dried: and over that another stupe spread with the same, so broad as to cover over the first, and up a pretty height about the stump. This latter must be snipt deep in the edges, that it may be smooth about the member. An ox-bladder, ready cut and wet, is to be turned over it, and a cross cloth next the bladder to hold the dressing steady. Then with a roller with two heads begin upon the stump, and roll up to the next

joint, and so again about the member, to retain your dressings firm. Then fasten it so as that it may not be capable of falling off. But before you make this bandage, you ought to apply your defensative over the parts above the stump. Some apply it before they begin the amputation ; but then it is apt to wrinkle and sit uneasy. In the dressings of the stumps you ought so to apply your medicaments, that you may, without much bungle, roll up the member. Then place it to the ease of the patient, one holding his hand some while upon the stump, &c. The third day take off the dressings ; and then you may cut the cross stitch and sprinkle the wound with *pulv. Galeni*, and dress it with this digestive : *Terebinth. &c. &c.* And apply to the bone a pledget of dry lint, or pressed out of *Sp. Vini* ; after the wound is digested, you may deterge with *Mundif. Paracel. &c.* Then incarn and cicatrize, as hath been said in compound wounds."

The above narrative, by Wiseman, is valuable to the medical student, as it furnishes him with a species of history of the original rude state of amputation. It enables him to retrace the steps through which this interesting operation has advanced ; the difficulties and uncertainties under which it has laboured ; and, by reference to its incompetencies, he may form an opinion of the provisions of our present mode of amputating.

The French Dictionary of Sciences furnish pretty much such another historical narrative. And which of the two accounts is the earlier is not for us to determine. Our purpose is with the facts in themselves.

It is not in anywise remarkable that the general history of the period of which we are now writing, should inform us that most of the patients died. It would rather surprise that any recovered ; at least until the needle and ligature were introduced by the celebrated Parè, who, it appears, was the first surgeon that ever secured the blood-vessels by ligature.

The censurable and barbarous method of the simple incision, without or with the cross-stitch, of which Wiseman in the above narrative makes mention, has been lately revived, in a few instances, by some surgeons of this state. But upon what principles I am incompetent to determine. It is to be hoped, however, that these are the last occasions that surgeons will spontaneously furnish of having their reputations shaded by a plan of conduct from which an enlightened understanding could have had nothing to expect, but from which experience had received the most bitter lessons. We have known, where surgeons addressed themselves to their patients in this sinister old style, two out of three, or three out of four, soon have to settle up the affairs of life ; and are persuaded that those who es-

caped, had but few reasons, when they consulted their feelings and their enjoyments, to envy the condition of the dead.

To the credit of the surgery of our state, there have been, at least for the last ten or fifteen years, but few operations of this character.

Tracing our art on the graduated scale of time, we descend from the less improved period of Wiseman to the not much more enlightened epoch of Young, and of whom but a few perishable fragments are to be collected.

AMPUTATION.

YOUNG, } “ As long ago as the year 1679
1679. } it was proposed by Jacob Young,
an English surgeon, in a treatise
entitled *Currus Triumphalis ex Terebinthino*,
to preserve a flap of flesh and skin, which was
to be folded over the bone, and which uniting
to the parts of the wound after amputation,
would effectually cover the bone. No traces
of the success of this method, however, can be
found till the year 1696, when a Latin dissertation
was published upon it by P. A. Verduin, an eminent surgeon of Amsterdam.”

“ The most sanguine expectations were formed of its success, and it was even thought that

the flap would prevent the *necessity of tying up the blood-vessels.*" *Encyclop.*

This operation by a single flap has, since the days of Young and Verduin, been revived under certain modifications. Instead of laying the flap up to the stump, and there confining it by suitable apparatus, immediately after the operation, the modern surgeons, who have adopted it, kept the surfaces asunder, and continued to dress them apart until they should suppurate and granulate ; they were then laid into coaptation, and an union effected. The vessels were secured by ligature, as is now customary. Among the surgeons referred to, O'Hallaran, an Irish surgeon of much character, took a distinguished part, and is mentioned as having censured a French surgeon in terms not very courteous, for the proposal of laying together and uniting surfaces recently divided.

A Mr. Loudham, a surgeon of London, is also said to have contributed to the original introduction of the operation by the single flap, and with expectations not dissimilar to those of Young, with whom he was cotemporary.

AMPUTATION.

VERDUIN, } It is uncertain whether Ver-
 and } duin and Sabourin had, at the
 SUBOURIN, } time they wrote, ever seen the
 in 1696, } publication of Young. But
 and 1702. } from the very exact accordance
 between the two methods, both as to general
 principle and particular circumstance, it is
 presumable that the works of the Englishman
 were not unknown to the Frenchmen. More-
 over, a considerable time had elapsed from the
 period at which Young published, and that
 when Verduin and Sabourin gave their senti-
 ments to the publick. And, notwithstanding
 the hostile attitude mutually, and almost con-
 stantly assumed by the British and the French
 people against each other, the intercourse be-
 tween the two nations, in a literary point of
 view, was very considerable, and, in the gene-
 ral, uninterrupted.

Both the English and French authority in-
 sist on the many advantages this operation,
 with a single flap, possesses over the former
 modes of amputation ; especially that it will
 supersede altogether the use of the astringents
 ordinarily had recourse to in those days, and
 the ligature. If it were generally true, or at
 all possible, that the flap, how carefully soever
 adjusted and adroitly secured by machinery,
 could supersede the ligature, the ideas of
 Young and Verduin would undoubtedly have

been valuable, and their methods an improvement, but no surgeon of the present day, without first abandoning his understanding, could admit the thing to be in any way possible.—The cases related of success under the above method I would rather pass over in silence, with the confession that my credulity has not a wide range.

By the publication of the inventions of Young and Verduin, it appears much emulation was excited among the French ; and about the same period (1739) two propositions were offered to the world, by M. Ravaton and M. Vermale. It does not appear that those surgeons were aware of each other's undertaking. Their improvements, real or imaginary, had an immediate reference to the thigh. They both recommended, that, in the place of one flap, there should be two, yet they materially differed, the one from the other.

RAVATON. } M. Ravaton made three incisions, each reaching to the bone.
 The first circularly, with a crooked knife, about four fingers breadth below where the bone should be sawed off. He afterwards, with a pretty large knife, made two incisions perpendicular to the first, commencing each at the point where the saw should be laid ; the one on the anterior, the other on the posterior part of the limb, care being taken

to avoid the principal vessels, he then *detached the two flaps from the bone.**

VERMALE. } M. Vermale executed but two
incisions, by which he made his
two flaps. When he had fixed
his tourniquet, which was in the ordinary way,
he surrounded the limb with two red threads,
at the distance of about four fingers breadth
from each other, the one at the place where
the bone is to be sawed off, the other where
the flap is to terminate. This being done, he
enters a knife before so as to penetrate to the
centre of the bone, and then glides on the
bone in such a manner as that its point will
emerge *directly* opposite to the place at which
it entered. Afterwards he forms his first flap,
carrying the *cutting edge of the knife along the
bone to the inferior thread*, giving it a rounded
or conical extremity, as the author phrases it.
In like manner he forms his second flap, on
the inner, if he commenced on the outer side.

Ravaton and Vermale finished their operations in a similar manner. They turned up

* “ M. Ravaton fait trois incisions pénétrantes jusqu’ a l’os :
premierement une circulaire avec le couteau courbe, à quatre
travers de doigts du lieu où on le doit scier ; il fait ensuite un
bistouri un peu grand, les deux autres perpendiculairement sur la
premiere, en commençant dans l’endroit où l’on sépare le mem-
bre, l’une à la partie antérieure, et l’autre à la partie postérieure
en évitant les principaux vaisseaux ; enfin il détache les deux
lambeaux de l’os.”—*Histoire de l’amputation par M. La Faye ;
De L’Académie De Chir. tom. ii. p. 251.*

their flaps, secured them by a split compress or band, cut what might remain of the muscles, divided the periosteum as is customary, and then used a saw of slender teeth. The ligatures were placed so as to hang out through the most dependent part, the flaps mutually approximated, and secured by straps, over which was an adhesive plaster, and the whole sustained by a bandage, so simple as not to need description.

Of these two methods of amputation, the former is the less exceptionable. It may possibly, with much labour and pain to the patient, and loss of time, be practised. But, besides the attention and address necessary to detach the flaps with any degree of decency from the bone, or bones, if it be on the leg, I object to the rectangular manner in which the perpendicular incisions intersect the circular cut. Meeting at right angles, the incisions will leave a flat stump. Another objection against this mode is, the muscles are left bare at their cut ends, there being no integument to cover and protect them, for the circular cut to the bone, through both integument and muscle, will leave the skin so that it will retract ; nor will the surgeon be able to bring the skin forward in such a way as to afford any protection to the exposed ends of the muscles.

The latter, that of Vermale, according to the letter of the text, labours under all the disadvantages of the one of Ravaton, besides some serious objections proper to itself.

In the first place, if the knife be entered in the front part of the thigh, so as to advance upon the centre of the bone, it can by no imaginable dexterity of the surgeon be so directed, as to glide round the lateral hemisphere of the bone, and come out in the opposite centre of the posterior part of the thigh. If the knife be entered in the centre before, it must come out far from the centre behind. The knife must pass along the side of the bone, if it is to go straight through. Even were the knife curved laterally, as has been suggested, it could not, but with great address and adroitness, be made to wind round the lateral hemisphere of the bone, and emerge opposite to where it enters, supposing it to pierce the middle of the front of the limb.

Again, if the cutting edge of the knife is to descend along the bone (*le long de l'os*) to the lower thread, a rounded, or conical extremity cannot be formed, except at the condyles, for it would constitute pretty much of a right angle. But I will agree, the writer meant that the incisions should mutually recede from each other, so as to come out on the opposite sides, just so as to touch on the lower thread. This,

from a plate referred to in the description, appears to have been the author's intention.*

But were the knife passed through the limb, a thing absolutely impracticable upon the plan indicated, and the flaps formed according to the manner mentioned by Vermale, they could not be turned up and detained, so as for the surgeon successfully to have access to the upper angle of the wound with his saw, except in an oblique way.

Such a thing, it is true, might possibly be practised on the lower point of the thigh, where the lateral muscular substance is not very considerable ; but of all points of the femur, the lower portion, and near the condyles, would be the most ineligible. The bone there is more spread out, and less suited for the first part of the operative process.—Added to this, the great artery and nerve would lie immediately in the way of the knife behind, and be exposed where there would be the least covering or protection, and, of consequence, would be liable to slough, and permit hemorrhage to occur.

The drawing, alluded to, represents the operation as performed at the condyles of the thigh, immediately where, I suppose, a prudent man would not be disposed to execute the affair. It does not appear that the suggestion of Vermale has ever been acted on, and, I

* See fig. 1, plate 1.

apprehend, for the most simple of all reasons ; the proposition is not reducible to practice. In short, this project of Vermale is nothing more than a puerile conceit, altogether below the dignity and gravity of a man of sense ; and the diagram furnished, as a mean of explanation, contravenes the graphical account in the main principles of the flaps, in the clearest manner satisfying every rational mind that the one was made to apologize for the defects of the other.

But even could the knife be passed in, directly upon the centre of the bone, and then, however possibly or dexterously, caused to emerge on the opposite part of the limb ; and further could, by the operation being performed at the condyles, be brought out so as to form two lateral flaps, as the knife is to be kept in contact with the bone, those flaps must be composed of both muscles and integuments, and be brought together in a wedgelike manner.

It were ridiculous, though, to conceive the operation to be practicable upon any other principles than those, since the days of Vermale, practised by Desault. The reader will perceive, when he arrives at this able surgeon's description of his own mode, the adaption.

The French have at no period of their professional history, given us any evidence on

which we can admit that they ever made two flaps, by dissecting the integuments from the subjacent muscles. Nor, indeed, that they ever made a simple circular flap by detaching the skin from the muscles. They have, as by Petit, made a circular separation of the skin, but it was not reverted, and again, after the operation was finished, brought over the face of the muscles in the form of a flap.

When, on the method of Desault, I shall offer my objections with respect to the lateral flaps, made of muscles and integuments combinedly, and leaving, of necessary consequence, a hollow wedge before and behind the thigh, requiring months to fill up with granulations and cicatrize.

No sooner was the plan of Vermale proposed to the academy than it was laughed at, and, in the most pointed manner, attacked and censured by the learned M. Louis, as it will, and must be, by every medical man who is favoured with a sound mind *

VERMALE. } * “ M. Vermale ne fait que deux incisions pour former les deux lambeaux. Quand il a posé le tourniquet, comme on le doit toutes les amputations, il entoure la partie de deux fils rouges à quatre travers de doigts de distance, l’un à l’endroit où l’on doit scier l’os, l’autre où doit finir la coupe des lambeaux. Il porte ensuite à la partie antérieure du membre la pointe d’un bistouri de sept pouces de longueur, il l’introduit jùsq’ à l’os, il la fait glisser autour de la circonference afin de la faire sortir par la partie opposée ; il coupe ensuite, au portant le tranchant du couteau le long de l’os jùsq’ à fil inférieur où il sépare le premier lambeau auquel on

AMPUTATION.

DICTIONNAIRE } “ When the first circular
DES SCIENCES, } incision is made (with the
1751. } amputating knife,) the sur-
geon receives the straight knife, or scalpel, in
order to divide whatever muscular substance
may remain about the bone ; or between the
two bones, if it be the leg or the fore-arm,
which is operated on. The muscles are then
drawn up by a retractor, with a slit in it, that
the bone may be sawed off. Care is to be ob-
served, by cautious proceedings at first, to fix
the saw ; afterwards the surgeon proceeds on
in the division of the bone more boldly, yet
with circumspection, lest the saw be entangled
or confined in the body of the bone ; espe-
cially towards the close, he is to be observant,
that he does not splinter the bone.”

donne par cette coupe une figure ronde par l'extrémité, ou co-
nique, comme dit l'auteur. Il forme enfin de la même manière le
second lambeau du côté intérieur de la partie, s'il a commencé
par le côté externe, et vice versa.”

“ M. M. Ravaton, et Vermeil finissent l'un et l'autre leur
opération de la même manière. Ils relevent les lambeaux et les
maintiennent relevés par le moyen de la compresse fendue, ils
coupent le reste des chairs, et séparent le périoste à l'ordinaire ;
ils recommandent de scier l'os avec une scie dont les dents soient
fines. Ils font la ligature aux vaisseaux, laissent pendre les liens
par la partie de la place la plus déclive, rapprochent les lambeaux
et les maintiennent par des languettes, à l'extrémité desquelles il
y a de l'emplâtre aglutinatif étendu, et par un appareil qu'il est
aisé d'imaginer.”—*Histoire par M. La Faye ; De L'Académie
De Chir. tom. ii. p. 252.*

“ Some practitioners make the circular incision at two cuts. By the first they cut through the skin and adipose membrane, about two fingers breadth below where they intend to make the second, down to the muscles ; they then *retract* the integuments and commence the second cut, close upon the edge of the retracted skin, which they carry circularly and perpendicularly down to the bone, and finish as above.”

In the Dictionary of Sciences is the first mention of the double incision among the French, but the reader will observe that the first cut is merely through the integuments, and that the integuments are only retracted : They are not dissected and turned up by way of making a flap. This repository of French literature gives some account of a manner of repressing hemorrhage by pressure, made with square folds of lint applied to the open mouths of the bleeding vessels. Stypticks had not at that time obtained so generally as in subsequent ages ; perhaps they were not in use at the period when the compress by lint, aided by a machinery, supposed to be appropriate and effectual, was relied on.*

DICTIONNAIRE } * “ Dès que l'incision circulaire est faite, on
DES SCIENCES, } prend le couteau droit pour couper les chairs
1751. } que restent autour de l'os, ou dans l'entre-
deux à la jambe et l'avant bras. On retrouve les chairs avec la
compresse fendue et on prend ensuite la scie que l'on appuie
sur l'os légèrement pour faire le première trace. On puit aller
après à plus grands coups ; mais toujours sans trop appuyer, de

AMPUTATION.

SHAW, } “ Then the patient being conve-
 1753. } niently situated, and the operator
7th Edit. } placed on the inside of the limb,
 which is to be held by one assistant above,
 and another below the part designed for the
 operation ; and the *gripe* being sufficiently
 twisted, to prevent too large a hemorrhage,
 the flesh is, with a single cut or two, to be se-
 parated from the bone with the dismembering
 knife. Then, the periosteum being also di-
 vided from the bone with the back of the
 knife, saw the bone asunder with as few strokes
 as possible. But when two parallel bones are
 concerned, the flesh that grows between them
 must likewise be separated before the use of
 the saw. When this is done, the gripe may
 be slackened, to give an opportunity of search-
 ing for the large blood-vessels, and securing
 the hemorrhage at their mouths, either by the
 actual cautery, *stitching them up*, applying vi-
 triol buttons, or the like. After this, a dry
 pledget of lint, or one dipt in spirit of wine,
 and sprinkled with *diapente*, may be applied to
 the stump. Then loosen the first ligature,
 and pull both the skin and flesh as far as you

crainte d'engager les dents dans le corps de l'os. Quand en est sur la fine, il faut aller plus doucement pour ne faire d'eclats.”

“ Quelques praticiens font l'incision circulaire au *deux tems* : ils coupent la peau et la graisse deux travers de doigts au-dessous du lieu où ils se proposent de scier l'os : ils font ensuite *retrousser* et assujetter les parties coupées pour continuer, à la niveau, l'incision jusqu'à l'os.”

can conveniently over the stump, to cover it, and secure them with the cross stitch, made at the depth of half, or three quarters of an inch in the skin. The thread used for this purpose should be double and strong. And now apply to the whole stump two large pledgets, first dipt in oxycrate and dried, and afterwards charged with astringents ; an assistant securing them with his hand. Upon these apply a plaster of deminium, or simple diachylon ; then pull on an ox-bladder, that has been first wetted in oxycrate, and afterwards a little dried ; and over all a cross-cloth, pretty tight, secured first with a single, and then a double roller. And, after all this, the gripe may be slackened so as to be made easy to the patient. Or it may be entirely taken away after he is put to bed, where he must lie with the stump somewhat raised ; an assistant for twelve or fourteen hours keeping fast the dressing with his hand, to prevent any violent hemorrhage. If it be found necessary, the patient may lose a quantity of blood at the arm, to keep down the fever that in this case is apt to rise ; and a composing draught, or opiate, may be exhibited occasionally. In three or four days the dressings may be removed, and proper digestives, mixed with astringents, applied. But at the first dressing an actual cautery, or some powerful styptick should be in readiness, in case of a violent hemorrhage. The exfoliation of the bone is now to be procured as formerly

directed ; after which the cure may be finished, as in case of a common wound or ulcer."

This advice given by Shaw, to abstract blood in case of fever, is well entitled to the attention of the surgeon. Especially so in cases where amputation is on healthy habits that have suffered some serious contusion, whereby perhaps not only the bone is fractured, but the integuments torn up, and the blood-vessels lacerated, so as to make immediate amputation necessary. Here the fever may run high, and signs of inflammation about the stump be considerable—And, if so, the lancet must be resorted to, and that as circumstances may require.

The lancet may also be proper in some few cases where the arteries divided, may be disposed to ossification, and where any extraordinary morbid action about the vessels might throw them into suppuration or gangrene, and thus a hemorrhage be produced. Or any considerable febrile action might prevent the regular healthy process of union, and endanger the patient's life.

What measures are proper to promote exfoliation, otherwise than as they add to the powers of health, I am at a loss to determine.—We can destroy parts by awkwardness and improper conduct, but the regeneration of lost substance is the work of the constitution,—

This exfoliation of the bone projecting beyond the surface of the healthy soft parts is inseparable from this mode of simple incision, or, in fact, every incision that does not leave muscle and skin sufficient to form a good and ample cushion for the end of the bone, thereby to fence out evils. The business of the surgeon is to prevent, not afterwards to remedy such evils as exfoliating ends of bones.

I am not confident that I understand what the writer means by stitching up the large blood-vessels. Does he suggest that the ligature be applied with a needle, and afterwards a stitch or two be made immediately across the mouth of the vessel ?

AMPUTATION.

SHARP, } “ Lay your patient on a table,
1758. } two feet six inches high, which is
7th Edit. } much better than a low seat, both
for securing him steady, and giving yourself
the advantage of operating without stooping,
which is not only painful, but inconvenient in
the other situation. While one of the assist-
ants holds the leg, you must roll a slip of fine
rag, half an inch broad, three or four times
round it, about four or five inches below the
patella. This, being pinned on, is to serve as
a guide for the knife, which, without it, per-
haps would not be directed so dexterously.
The manner of rolling has always been per-

pendicular to the length of the leg, but having sometimes observed, that though the amputation at first be even, yet afterwards the gastrocnemius muscle contracting, draws back the inferior part of the stump more strongly than the other muscles can do the rest of it. I have lately, in order to preserve the regularity of the cicatrix, allowed for this excess of contraction, and made the circular incision in such a manner, that the part of the wound which is on the calf of the leg is a little farther from the ham, than that on the shin is from the middle of the patella. In the mean time, one of your assistants must carry a strong ligature round the thigh, about three or four inches above the patella; which, passing through a couple of slits in a square piece of leather, he must twist with a tourniquet, till the artery is sufficiently compressed to prevent any great effusion of blood; and, to do it more effectually, he may lay a bolster of tow, or linen, under the ligature, upon that part where the artery creeps. It will also be a little more easy to the patient, to carry a compress of linen, three or four times double, round the thigh, on that part where the ligature is applied, in order to prevent it from cutting the skin."

"The course of the blood being stopped, you must begin your incision just below the linen roller, on the under part of the limb, bringing your knife towards you, which at one

sweep may cut more than the semicircle ; then beginning your second wound on the upper part, it must be continued from the one extremity to the other of the first wound, making them but one line. These incisions must be made quite through the *membrana adiposa*, as far as the muscles ; then taking off the linen roller, and an assistant drawing back the skin as far as it will go, you make your wound from the edges of it when drawn back, through the flesh to the bone, in the same manner as you did through the skin. Before you saw the bones, you must cut the ligament between them with the point of your knife, and the assistant, who holds the leg while it is sawing, must observe not to lift it upwards, which would clog the instrument ; and, at the same time, not to let it drop, lest the weight of the limb should fracture the bone, before it is quite sawed through."

" When the leg is taken off, the next regard is to be had to the stopping the blood, which must be effectually done before the patient is put to bed, &c. There is no method for this purpose so secure, as taking up the extremity of the vessels with a needle and ligature, in the following manner. As soon as the amputation is performed, the assistant must loosen the tourniquet for a moment, upon which the orifices of the arteries will appear by the issue of the blood. The operator having then fixed his eye upon one of the largest

vessels, passes a crooked needle through the flesh, a little more than a quarter of an inch above the orifice, and about the same depth in such a direction, as to make nearly one third of a circle round the vessel : then withdrawing the needle, he a second time passes it into the flesh and out again, in the same manner, and about the same distance below the orifice of the vessel : by this means the thread will almost encompass the vessel, and when it is tied, (which should be done by the surgeon's knot,) will necessarily inclose it within the stricture. All the considerable arteries are to be taken up in the same manner."

" This is a much better way than using the artery forceps, where the vessels are apt to slip away out of the ligature ; and, as to styptick applications, their want of safety is so well known now, that the use of them, in hemorrhages from large vessels, is almost universally rejected ; though it is thought by several surgeons, who have experienced the virtue of agaric, that it will be found to be a more powerful astringent than any hitherto discovered."

" It sometimes happens, in a large stump, that there are ten or more vessels requiring tying, which done, you must apply loose dry lint to the wound ; or, in case the small vessels bleed plentifully, you may throw a handful of flour amongst the lint, which will contribute to

the more effectual stopping up their orifices : Before you lay on your pledgets, you must bind the stump, and begin to roll from the lower part of the thigh down to the extremity of the stump. The use of this roller is to keep the skin forward, which, notwithstanding the steps already taken to prevent its falling back, would in some measure do so, unless sustained in this manner. The dressing may be secured by the cross cloth, and gentle bandage ; and the method of treating the wound, may be learnt from what has been said with respect to recent incised wounds."

From the above description, by Mr. Sharp, the reader may understand pretty fully the general usage of his time. The English surgeons differed but little, or not at all, from the method just detailed. Except in some slight matters of preparation, indeed, their plan was the same as that of Sharp. The general outlines of Heister's, were not so different as to constitute even a well marked variety.

This operation by Sharp, countenanced as it was by most of the chief surgeons of England, became pretty general throughout that country, and, as America derived many of her customs from England, was that which, for the most part, was followed in this State during its early periods. It is that plan which has obtained, among medical men, the distinctive appellation of the Double Incision.

Sharp, in his description, is evidently speaking of the operation when applied to the leg, but it was the same when used on the thigh, with the difference of there not being two bones in the thigh, of consequence the instructions in regard to the interosseous ligament would be inapplicable in an operation on this part of the extremity.

Some surgeons, after the face of the stump was well covered with soft lint, fully charged with flower, laid their adhesive straps over in a stellated way, drawing the skin down so as to cover the stump as well as they could, which was not at all ; for the integuments will gradually gather up and retract, whether the surgeon's attempts to confine them be by straps, or the more uncompassionate invention of the cross-stitch.

AMPUTATION.

Gooch, } “ After making the *double circu-*
1766. } *lar incision*, with the utmost exact-
ness, in the usual manner, the
operator may use either of the short knives
referred to in plate 14,* or a common incision
knife, to divide the muscles quite round,
about two inches up, close to the periosteum,
and then put on the retractor, adapting the
hole of it well round the bone upon the peri-
osteum, desiring an assistant to draw up the

* See Gooch's plates.

muscles equally and regularly with it as far as shall be thought necessary ; after which the bone is to be sawn as near the retractor as possible, without paying any regard to the periosteum," &c. &c.

What Gooch terms here the *double circular incision*, is, I apprehend, that described by Mr. Sharp, and several of the French surgeons, to which the reader will refer. It does not appear, that after the integuments were divided circularly to the muscles, they were detached for any distance up from the muscles, as afterwards became the practice. He speaks of his double circular incision as being done in the usual manner, alluding, it may be presumed, to the manner observed by Sharp and Bromfield. This also may be inferred, from the muscles being separated from the bone to the great extent of two inches, and from his plasters being applied on the end of the stump, so as to "form the figure of an asterisk." His cicatrix, at the end of the stump, was nearly circular, and, in some instances, "but a few lines broader than the diameter of the end of the bone."

The separation of the muscles from the bone, is that part of the Roman mode of operation, as recommended by Celsus, which particularly entitles it to the attention of the reader. It does not appear from the records of surgery, so far as they have come under my

view, that this method of detaching the muscles from the bone, to any given distance, before the retractors were applied, and the bone sawed off, was practised by any surgeon from the days of Celsus, until revived by Gooch. This separation of the muscles from the bone, is generally styled the third cut, and from it, the operation by Gooch has derived the distinction of the Triple Incision. And, although Kirkland and others, since the introduction of the distinct three cuts, by Gooch, have added a fourth, the separation of the skin from the muscles, and the turning up of the detached skin, the operations of Kirkland, and B. Bell, are still said to be performed by the Triple Incision, the first circular incision of the skin, and the detachment of it from the subjacent muscles, being considered as one continued cut.

AMPUTATION.

BROMFIELD, 1773. } “ As soon as the tape is thus applied, the tourniquet is to be screwed tight: the circular incision through the integuments being made by the dismembering knife, if any little parts of the integuments still adhere to the muscles, they should be set at liberty with the point of it, so as that the skin should slip easily over the muscles. The assistant must then draw up the skin as high as possible, which the operator may assist with his fingers.

The knife is then applied close to the edge of the integuments thus drawn up, and carried quite through the muscles down to the bone, in a circular manner as before. The periosteum is then to be cut through with the catling, as high as possible on the bone, and, at the same time, the lower edge of the membrane should be stripped a little downward with the knife, to make sufficient space for the application of the saw."

It is a little extraordinary that Bromfield should continue the practice of Sharp, and not even so much as advert to the improvement revived by Gooch. It is scarcely possible that he should not be acquainted with it, as Gooch's work had been, when Bromfield wrote, seven years in the hands of the publick. Where there is a material and valuable suggestion in a precedent writer on a science, if it be not obligatory on a subsequent author, it were at least decorous in him, to make some mention of it. Silence, by men of honour and sense, is always considered to be, if not disreputable, at least but a feeble weapon, in the hands of envy or timidity. There are generally more men of reading than one, in every department and in every age. And not only so, but there are, for the most part, more authors than one; of consequence, a valuable suggestion is not apt to be lost, although passed by through the discourtesy, or restricted reading, I will not say immorality, of a competitor. Silence may

awaken suspicions as to understanding, and sometimes as to morals, but can never set aside claims founded in good sense and correct principle. The meandrous stream of truth will ultimately wind through the mole-hills of folly, and meet the eye of observation.

I do not limit the above animadversions to Mr. Bromfield ; they may be applicable in other directions, and to other surgeons ; especially to those that engage in the occupation of historians. Such should recollect, that a historian ought to be honest, although nature forbid that he be wise, or indolence that he be learned. The rays of science are not to be obscured by the *negative* extinguisher of silence, although placed by the disingenuous hand of presumption.

AMPUTATION.

ALANSON, } “ Apply the tourniquet in the
1779. } usual way ; stand on the outside
of the thigh, and let an assistant
draw up the skin and muscles, by firmly grasping the limb circularly with both hands. The operator then makes the circular incision as quickly as possible, through the skin and membrana adiposa, down to the muscles. He next separates the cellular and membranous attachments with the edge of his knife, till as much skin is drawn back as will afterwards, conjointly with the following division of the

muscles, cover the surface of the wound with the most perfect ease."

"The assistant still firmly supporting the parts as before, apply the *edge* of your knife upon the inner edge of the musculus vastus internus, and, at one stroke, cut obliquely through the muscles upwards, as to the limb, and down to the bone ; or, in other words, cut in such a direction as to lay the bone bare, about two or three fingers breadth higher than is usually done by the common perpendicular circular incision. Now draw the knife towards you, so that its point may rest upon the bone, still attending to keep it in the same oblique line, that the muscles may be divided all round the limb, in that direction, by a proper turn of the knife ; during which, its point is kept in contact with, and revolves round, the bone."

"The part where the bone is to be laid bare, whether two, three, or four fingers breadth higher than the edge of the retracted integuments ; or, in other words, the quantity of muscular substance to be taken out, in making the double incision, must be regulated by considering the length of the limb, and the quantity of skin, that has been previously saved by dividing the membranous attachments."

"The quantity of skin saved, and muscular substance taken out, must be in such an exact proportion to each other, as that, by a re-

moval of both, the whole surface of the wound will afterwards be easily covered, and the length of the limb nor more shortened than is necessary, to obtain this end. However, it is to be observed, that the more muscular substance we save, by fully giving the oblique direction to the knife, instead of dividing the membranous attachments, the better."

Mr. Alanson now gives some directions for the use of the retractor ; for securing the divided arteries with ligatures ; and for the application of the flannel roller. Afterwards he proceeds thus : "You are now to place the skin and muscles over the bone, in such a direction, as that the wound shall appear only *in a line, with the angles at each side* ; from which points the ligatures are to be left out, as their vicinity to either angle directs. The skin is easily secured in this posture, by long slips of linen, or lint, about two fingers in breadth, spread with cerate, or any other ointment. If the skin do not easily meet, it is best brought into contact by slips of linen, spread with stitching plaster. These are to be applied from below upwards, across the face of the stump, and over them a soft tow pledget, and compress of linen, the whole to be retained by the many-tailed bandage, with two tails, or slips, to come from below upwards, to retain the dressings upon the face of the stump."

The excavation of the muscular substance ; the directing the line to be from the angles at each side ; and the turning the under, or posterior flap up, give this operation of Mr. Alanson a character, decidedly among the most exceptionable of modern times. The flap being turned up from below, and thus secured, will, of necessity, serve as a recipient for all the serum and blood that may be effused from the recently divided vessels.

The reader will see more on this subject, when what I may say, in respect to Mr. S. Cooper, is before his eye.

AMPUTATION.

KIRKLAND, } “ Nevertheless amputation
1786. } may become necessary from an
extension of the caries, both
above and below the joint ; an approaching
colliquative fever ; and a variety of other
causes : but this operation should never take
place, without a consultation of those whose
experience in medical surgery, enables them
to judge what is most expedient to be done,
for the happiness or safety of the patient :
and, when amputation is the miserable, but
only remedy, it should be set about with as
little parade as possible. There is no need
either for the barbarous dress, or frightful ap-
paratus, sometimes used on this occasion. A
much smaller strait knife, with a sharp point,

which some have recommended, is less alarming, and much more handy, than the large one, that has in general been used ; especially, as it will do every part of the operation.—Whereas, if we use the other, we must change our instrument as we go on. A silken handkerchief makes the best, and most easy tourniquet I ever saw, and is less terrifying to the mind of the patient than any kind of formal machine ; and, if there be an abridgment in the saw, it will not be less useful. The operation should provide for the end of the bone being deep buried among the muscles, and for these being covered with the skin ; for without both these precautions, pyramidical stump has been a common consequence. This is prevented by making a circular, or common flap, as the part on which the operation is performed may require ; by preserving skin enough in the operation to exactly cover the wound, and by drawing up the flesh with a retractor, as high as is necessary for sawing through the bone ; and we see where nature divided the skin and muscles below the elbow, an accidental flap was sufficient to forward and make a good cure. But the success of the operation depends on the after-treatment ; for unless this be well managed, it is not of any great consequence by what method the limb is taken off ; as the healing of the stump will commonly end in an awkward manner.”

“ The want of success, which brought the first trials of the flap operation (Young) into discredit, was owing to the after-treatment not being properly pursued ; because it has since repeatedly succeeded ; not always indeed by an immediate union of the whole of the divided parts, but connection so far commonly takes place, that by proper dressings and bandage, a much shorter time elapses before the cure is complete, than when this mode of operating is omitted. More than thirty years ago, my friend Mr. Fisher, and myself, in making the double incision, to take off the leg above the knee, agreed to divide the muscles, slanting all round towards the bone ; and afterwards drawing up the flesh, I readily sawed out the bone from under the muscles, farther than could be done by the usual method ; but I did not make those advantages of this mode of operating I might have done, owing to my following the common practice of applying lint and flour to the wound. However, I always began to apply a mild digestive balsam at the second dressing, and not cramming the ulcer with this, or any other application, its sides gradually approached each other, and, by the assistance of a double headed roller, as soon as inflammation would admit of its being used, I generally had the satisfaction of seeing this business terminate better than when the operation was performed in the common manner. When Mr. Gooch recommended a third incision, with a small knife upwards round the

bone, and drawing back the muscles with a retractor, I tried this method, and found I could leave the end of the bone covered any depth I chose, with the utmost facility ; but I always took care to preserve skin enough to cover the wound, I did not yet think of curing by the first intention. However, having learned that the small arteries would contract under any application, and that dry lint is an extraneous body, which embarrasses nature with difficulties ; I dressed with a mild digestive balsam, lightly introduced into the cavity from the beginning, and bringing down the muscles with the usual bandage. A mild ointment was then applied outwardly, and, instead of raising the stump with a pillow, I laid it flat upon a doubled sheet, and suffered the patient to lie in a posture most easy to himself ; by which method, assisted with the double headed roller, my first patient in this way, with a very thick thigh, was perfectly cured in less than five weeks, with every advantage we could desire ; and for many years I pursued the same method with equal success."

" Much about this time Mr. O'Hallaran revived the flap operation, by uniting the parts, after *digestion*, &c. had taken place ; and, after him, Mr. White practised the flap operation above the ankle, in nearly the same manner. These steps probably gave rise to the circular flap above the knee. To the after-treatment Mr. Alanson introduced, Mr.

Myers has since assisted much, in bringing this business towards perfection. Their attempts to cure as much as possible, by the first intention, is a most rational practice. Covering the wound with the skin, is certainly the most natural application ; and, when it is placed in contact with the subjacent parts, adhesion and inosculation are well known to take place in a few hours. Great inflammation, that disturbance in the animal economy, which irritation, the progress of digestion, and the subsequent absorption of matter produce, are prevented, as some of our principal writers have long since observed. It only therefore remains for us to compare the double and triple incision, and see which method is most likely to answer these purposes."

" When we proceed upon the plan of making a circular flap, by cutting out part of the muscles, those which remain on the sides are brought over the extremity of the bone ; the skin, which at first lies loosely over the sides of the muscles, adheres in a little time ; nature supplies the lost substance, and a much speedier cure, and less inconvenience follow, than when the patient is treated in the common manner."

" When we pursue the method of removing the limb by a triple incision, after cutting through and drawing, or *turning up the skin*, we divide the muscles ; beginning close to the

edge of the skin, by a perpendicular circular incision, and while the flesh is drawn gently backward by the retractor, it is separated from the bone by the point of the knife, as far up as is necessary to leave a sufficient covering, when the parts are healed.. By this means we make a circular flap of the whole muscles, without loss of substance, that will unavoidably close down over the end of the bone. The skin and muscles are brought face to face, laid in close contact ; very considerable arteries, with the assistance of a little pressure, are as effectually stopped by the skin being thus applied, as they could be by the application of agaric or sponge ; and there will be nothing, except the ligatures securing the large arteries, or a bad habit of body, to prevent the first intention, or an immediate adhesion of the skin and muscles taking place. But supposing the amputation to be made above the knee, we shall be better understood by drawing a few lines.”

“ In the first instance, the skin is divided round the limb at *a*, and drawn, or *turned back to b*.* The slanting incision is then made, and the bone sawed off at *c*. Afterwards, the skin from *b* to *a* being brought over the muscles, of course turns them inward, to make a covering for the end of the bone ; in which, it must be observed, we differ from other flap operations,

* See Kirkland's plates.

where, instead of robbing the part of muscular flesh, we preserve it entire, to make as thick a covering as we conveniently can. And, besides, by taking away part of the muscles, the skin cannot be placed in so close contact as when the surface is level ; and, if they are not placed in close contact, they cannot immediately unite. On the contrary, when the operation is performed by triple incision, after dividing the skin at *d*, and drawing, or *turning it back* to *e*, the muscles are divided by a perpendicular incision. The third incision is then made close along the bone to *f*, where it is sawn off, by which means the circular is not weakened, the bone is buried deep among all the muscles, and the skin is laid close to a smooth surface. I know either method will do better than that which has been in common use ; but it may be observed, of the latter, that adhesion more readily takes place, a better covering is formed, and the patient recovers with the utmost ease that can be expected : and what may seem strange, we will call in the evidence of Mr. Alanson in favour of such practice, executed under rather unfavourable circumstances."

" Before he devised his present plan of operating, he amputated a man's leg, on account of a white swelling of the knee, by the double incision. The muscles being divided perpendicularly to the bone, many vessels were tied, and the wound was dressed in the usual man-

ner, with bandage, dry lint, and pledgets of digestive, &c. But in a few hours afterwards an hemorrhage occasioned his taking off the dressings, when he was struck with the sufferings of the patient, upon the lint, which had formed a firm adhesion with the surface of the sore, being separated. Wherefore, after the bleeding arteries were secured, and the circular bandage re-applied, instead of dry lint, he placed the skin over the surface of the wound, as far as it would go, and dressed the whole with digestive pledgets."

" Upon the fourth day after the operation" (says he) " I changed the dressings, which all separated with the most perfect ease ; the discharge was very small, and the skin was over the wound, exactly as I had left it, and the whole in a very kind state, respecting inflammatory tension. In short, the skin soon formed such adhesions as fixed it where it was first placed ; the discharge was uncommonly moderate through the whole cure, and by continuing the bandage, to support the parts, with soft gentle dressings, the stump perfectly healed in twenty days. The cicatrix was in the centre of the stump, and so small, as perfectly to be covered with a shilling ; and as the old skin formed so considerable a portion of the extremity of the stump, and there had been so small a waste of the adipose and cellular parts, in consequence of the small suppuration ; the whole looked very plump and full,

and formed the best cushion to walk upon I had ever seen."

" In another place he says, the more muscular substance we save the better, and had the third incision been made, would it not have met these ideas, and been a proper provision against any inconvenience from the bone being left too near the skin ? Nevertheless, Mr. Lyon first suggested the design of omitting all intermediate dressings, and placed the skin in a line on the face of the stump, with a view of uniting the whole by the first intention. Practice evinced the rectitude of this proceeding, and the inconveniences Mr. Alanson points out, from using dry lint after amputation, holds good in every wound to which it is applied : Thus, by uniting Mr. Gooch's method with the modern practice of curing by the first intention, we add some improvement to the direction given by Celsus, seventeen hundred years ago ; and, by adhering strictly to simplicity, we certainly avoid the greatest part of the danger attending amputation. If the limb be removed by a triple incision, and coagula of blood sponged away, the wound being a level surface, may be covered with the skin with the utmost exactness, and a pressure made upon it with the palm of the hand, will not only occasion adhesion, but suppress any hemorrhage from the muscular arteries, in a very short time : after which, strips of sticking plaster, the diachylon oint-

ment, and small bolsters on each side the lips of the wound may be applied. But, above all, the muscles should be placed in a relaxed state, and preserved in that situation perfectly free from motion, (as says Mynors,) otherwise, by suffering them to be put into motion, the union may be disturbed or destroyed, and digestion the consequence. But even supposing this to happen, it will be brought about with less inconvenience to the patient, than if any extraneous body had been applied ; and the parts will be readily made to unite, by bandage applied day by day."

" The old method of drawing out the arteries, and tying them bare without any adjoining substance, obviates all the objection against the ligature ; but a thin waxed thread does not appear to me to be the most eligible ligature for the purpose ; for unless the vessels be tied with the greatest nicety, it may cut off the end of the vessels, which, being closed only *to the next collateral branch*, may sometimes occasion a fresh hemorrhage, and this is said to have happened. Wherefore, it is impossible such an event should follow, if we tie them with long untwisted twelve-penny flax, as I long since recommended in my Essay on Hemorrhages from divided arteries. It lies upon the vessel as soft as lint would do, comes away in a shorter time than the common ligature, gives less interruption to the healing of the wound ; and the length of time I have em-

ployed it, enables me to speak of its utility with confidence."

"I have disapproved of the woolen cap, because it is true, that it pulls back the edges of the wound, and gives pain upon being used. The tail-bandage is void of these inconveniences, but the common four square piece of flannel, slit at each corner, is equally handy and useful. These, however, ought only to be employed, where we cannot make use of better assistance : for a certain degree of pressure promotes curing by the first intention, more than any other remedy ; nor can it be made with any bandage equal to a roller with two heads. But when the operation is performed in the thigh, or leg, this is inapplicable, till the adhesion, &c. has firmly taken place ; because moving the limb to use it before that period, would over-balance its good effects."

"Half the diameter, appears to be the due proportion to be observed in preserving skin in the amputation of various sized limbs, where none of the muscles are cut out ; but when excavated, less will be sufficient. In the lines above drawn, I have supposed the limb to be nine inches in circumference, and the bone to be sawn off about three inches from the first incision, one half of which is skin left without muscles ; in which it will appear that the wound will be completely covered : but

the eye will generally measure the length of the skin necessary for this purpose, and this will depend upon practice. When the triple incision is made, the bone may be brought out from under the muscles any length the surgeon pleases ; though moderation should be observed, that a sinuous ulcer may not be the consequence. Nor should the end of the bone be left uncovered with the muscles, less than *an inch and a half*, because more or less of contraction of the muscles will unavoidably take place. I agree with those who assert, that if more skin is saved than is necessary to cover the face of the wound, it proves inconvenient, not only in being superfluous, but it will not lay so close as it ought to do, to prevent an hemorrhage from the muscular arteries. It is better if the lips can just be drawn together with tolerable ease : and by making an incision through the skin on each side the thigh, as the annexed plate directs, we determine the exact breadth of the skin to be saved ; and we have not only a better opportunity of separating the cellular membrane, and turning up the skin, but we prevent its *puckering at the corners*, and a more level surface is produced."

" It may be observed, that in the leg, we preserve some skin upon the shin, to be turned up before the bone is sawed off, sufficient to meet the flap, and form a *horizontal line across the stump*. If the foot be taken off at the

place marked in the drawing, the tendon, as we have already observed, must be removed, to prevent the inconveniences that would arise from its being in the way. In a long stump, if the boot be well finished, it makes a lateral pressure all the way up, which gives steadiness ; and the machine I have used having a regular shaped foot, with a spring in the ankle, which raises the toe in walking, the patient uses both legs in the same manner."

" In the thigh, the ligatures may hang out in the front. In the leg, on the sides ; and one or other of these methods may be pursued in most amputations of larger extremities ; and perhaps complete, as nearly as possible, the wishes both of Wiseman and Sharp."

No comment on Mr. Kirkland at present would be proper, as I shall embrace whatever is in my opinion suitable, when speaking of my own proposition. I will merely remark now, that when the operation is in the thigh, and the ligatures hang out in front, they do not occupy the angles, but come out in the middle of the flaps. The reader will examine the original plates of Kirkland, to be found in his most valuable work. I have copied his plates only so far as to represent the general principles of the operation.

AMPUTATION.

BENJ. BELL, } “ In amputating either the
 1788. } thigh or leg, the patient should
 be placed upon a table of an
 ordinary height, with the leg properly secured
 and supported by an assistant, sitting before
 him. The other leg should likewise be sup-
 ported, at the same time the arms should be
 secured by an assistant on each side, to pre-
 vent interruptions during the operation.”

“ The flow of blood should be stopped by
 the application of the tourniquet, in the man-
 ner we have mentioned in the first volume of
 this work. And, as it is a matter of impor-
 tance to have the instrument placed as near as
 possible to the top of the thigh, the cushion
 placed upon the artery should reach the
 groin.”

“ This becomes absolutely necessary when
 the operation is to be performed on the upper
 part of the limb : but it may likewise be done
 with safety, where it is to be taken off imme-
 diately above the joint of the knee : and we
 may just observe, with respect to the most
 proper place at which the thigh should be am-
 putated, that no more of it should be taken
 away than is rendered necessary by the dis-
 ease ; for the more of it that is left, the more
 useful it proves.”

“ An assistant should now be directed to grasp the upper part of the limb with both hands, and to draw up the skin and cellular substance as far as possible. While they are in this state of tension, the operator, standing on the outside of the patient, should divide them with a circular incision down to the muscles. This may in general be done with one stroke of the amputating knife ; but in large limbs it is easier done at twice . The assistant continuing to draw the teguments upwards, the cellular substance connecting them to the muscles beneath should be divided with the edge of the knife, till as much of the skin is separated as the operator thinks will cover the stump completely.”

“ The skin being still drawn lightly upwards, the muscles should be divided close to the edge of it, down to the bone, by one perpendicular stroke of the knife, beginning with the upper part of the large muscles, on the inside of the thigh, and continuing the incision round through those beneath, and on the outside, until it terminates where it commenced. During this part of the operation, some attention is necessary to avoid the edge of the retracted skin. But it may always be done if the operator be upon his guard, for he may, with little difficulty, have his eye upon the course of the knife from first to last : nor can this part of the operation be done with safety in any other manner. Even where different

assistants are employed to protect the skin, it will be apt to be wounded if the operator does not follow the knife with his eye."

" In the *usual method* of operating, the bone would now be sawn across at the edge of the retracted muscles. But we are more certain of having a good stump, if the muscles be previously separated from the bone for the *space of an inch* ; and it is easily done by inverting the point of a common amputating knife between them, and carrying it freely round from one side of the limb to the other. This being done, the muscles and teguments should be drawn up as far as the muscles have been separated from the bone. And it is easily done, either with a bit of slit leather, or with the iron retractors in plate LXXXIV. The periosteum should now be divided at the place where the saw is to be applied, and it should be done with one turn of the knife ; for where much of it is scraped off, very tedious and troublesome exfoliations are apt to ensue. The knife should therefore be carried round the bone directly beneath the retractors. At this place the saw should be applied, and with long steady strokes the bone should be divided. In performing this part of the operation, the assistant holding the leg should be directed to support it with much equality ; for if it be raised too far, the motion of the saw will be impeded, while the bone will be apt to be splintered, if it be not sufficiently raised. Any

points of splinters which may be left, should be immediately removed with the nippers."

"The retractors should now be taken off ; and the trunk of the femoral artery, being drawn out with the tenaculum, a sufficient ligature should be made upon it before the tourniquet is loosened. But as the muscular branches of this artery cannot be discovered as long as any compression remains upon them, the screw should be immediately untwisted, so far as to remove it entirely. All the clotted blood should now be removed from the stump, with a soft sponge soaked in warm water, and every artery that can be discovered, should be secured with a ligature, care being taken to leave the ends of the threads of a sufficient length to hang out without the lips of the wound."

"The blood-vessels being all secured, and the surface of the wound cleared of blood, the muscles and teguments should be drawn down till the skin completely covers the stump ; and should be retained in this situation by an assistant, till a flannel or cotton roller, previously fixed round the body to prevent it from slipping down, be applied in such a manner as to support and fix them. For which purpose, it should be passed two or three times, nearly in a circular direction, round the top of the thigh ; and should afterwards, with spiral turns, be brought down near to the end of

the stump, of such a tightness as to prevent the muscles and skin from retracting, without compressing them so much as to prove painful, or to impede the circulation. Here the roller should be fixed with a common pin, while as much of it is left as will pass two or three times round the stump, for a purpose to be afterwards mentioned."

" The ends of the divided muscles being placed with as much equality as possible over the bone, the edges of the skin must be laid exactly together, so as to form a straight longitudinal line along the centre of the stump. When there is only one or two ligatures, they should be left out at the *inferior angle of the wound* ; but when there are several, they should be divided between the two angles, to prevent the parts from suffering, by a large extraneous body fixed at any one place."

" While an assistant retains the edges of the divided skin in exact contact, two or three slips of adhesive plaster should be laid across the face of the stump, to preserve them nearly in this situation ; and the whole surface of the stump should now be covered with a large pledget of soft lint, spread with Goulard's cerate, or the common calamine cerate of the Dispensatories. Over this there should be placed a soft cushion of fine tow, with a compress of old linen. For the purpose of retaining them, as well as with a view of making a

gentle pressure upon the stump, a slip of linen, of three inches in breadth, should be laid over them ; and should run directly across, and not from above downwards. On being properly placed, the remaining part of the roller should be employed to fix it, by passing it two or three times round the stump ; and the pressure formed by the cross strap, may afterwards be increased or diminished at pleasure, by drawing it with more or less tightness, and fixing it with pins to the circular roller."

" While we apply the roller, the tourniquet should be removed, and replaced immediately when the stump is dressed. If left loose, it gives no uneasiness ; and it enables the attendants to check any hemorrhage which may happen. A circumstance which merits attention for several days after amputation of any of the extremities."

" The patient should now be carried to bed ; but instead of raising the stump to a considerable height with pillows, as is usually done, it should be laid somewhat lower than the rest of the body. For this purpose, the bed should be made with a gentle declivity, from above downwards, and nothing should be put below the stump but a little fine tow."

" To prevent the patient from moving inadvertently, as well as to guard in some measure against the effects of those spasms which

often prove troublesome after this operation, I commonly employ two slips of linen, or flannel, to fix the stump down to the bed. It is easily done, by laying one across near the extremity of the stump, and another near to the root of the thigh. They should be pinned to the circular roller round the limb. Or they may be tied to it by pieces of small tape, previously sewed to the bed or the mattress ; which answers better than a feather bed, for any patient that is to be long confined. A basket, or hooped frame, should now be put over the stump, to protect it from the bed-clothes ; and, whether the patient complains much or not, I make it a constant rule to give him an anodyne, by which he remains quiet and perfectly easy through the remainder of the day, instead of being restless and distressed, which he is otherwise apt to be."

The objection to Mr. B. Bell's method, and the only one I have to offer, is, that the angles will be puckered and unseemly ; the stump cannot be regular and uniform ; and possibly the angles may afford occasion to sinuous abscesses, by uniting in part by the first intention, and detaining the fluids between them, at least the lower one, and the face of the divided muscles.

Every impediment that could possibly interfere with an early and complete union of parts, should be carefully guarded against, and

retrenched from the plan of operation. No operation can be too simple, provided its simplicity does not affect its efficiency. Nor is any circumstance, how minute soever, below the regard and attention of the operator.

AMPUTATION.

DESAULT, } “ All the apparatus of ampu-
1798. } tation being at hand, and the
artery secured ; Desault, placed
on the right of his patient, embracing the soft
parts of the inside of the thigh with his left
hand, cut through them with a straight knife
until he arrived in front of the bone, he then
forced his knife on, gliding on the bone,
through the limb, so as for the knife to ap-
pear on the posterior part of the limb.—
He now drew the knife down, and obliquely
outward, so as, by coming out at the sur-
face, to form a flap of about four fingers in
length. The flap was turned up, and the ves-
sels secured. He again entered his knife on
the outside, having laid hold on the soft parts,
and cut out the external, as he had done the
internal flap. This being accomplished, he
divided the soft parts which had escaped the
two previous cuts ; turned up the flaps, and
sawed off the bone as near as possible to their
base.”*

* “ Desault, placé au côté droit, et tenant embrassées avec la main gauche toutes les parties molles du côté interne de la cuisse, traversa ces parties avec un couteau droit, qu’il enfança

Here we have from Desault, something in a form reducible to practice, not unlike the method proposed by Vermale ; with this difference, that the knife glides along the side of the bone, and, by coming obliquely out to the lateral surface, forms a flap of a length correspondent to the obliquity. The two flaps being formed, the intermediate soft parts are divided, the bone sawed off, and the flaps brought into contact as regularly as may be.

There is much to be objected to in this plan of Desault, published by Bichat. Yet, perhaps, it is among the most improved of those that are strictly and properly of French origin and invention. In their various and numerous methods of amputation, the French have been by no means fortunate. They are fond of operations, and indulge frequently in the use of the knife ; but, with respect to the operation now under examination, either talent, or good fortune, lies on the opposite side of the channel. The opinion I express is of conviction, and not of feeling. I admire the French learning, and venerate their genius.

au-devant de l'os, et dont il fit sortir la point à la partie postérieure de la cuisse, ou la faisourt glisser sur le femur ; puis, en coupant obliquement en bas, il forma un lambeau d'environ quatre pouces de longueur, &c. Le chirurgion fit ensuite, de la même maniere, le lambeau externe, &c. Ou coupa le plus haut qu'il fut possible, les parties molles qui avoient échappé aux deux premieres scetions, et scia le femur contre la base des lambeaux."—*Desault*.

The wedgelike form, necessarily resulting from the regular approximation of the two flaps, will render the stump altogether unfitted for the future support of the body, either when standing or walking. To which impairment, the reader may very justly add, the length of time that will be required for healing the irregular hollow which must remain after the operation, both before and behind the limb. This irregular opening must fill up by regular granulation, and be cicatrized. A succession of operations will be thus involved, which will demand weeks or months for their accomplishment. It also might be suggested, that there will be required no inexperienced hand, successfully to conduct the knife through the limb, and construct the flaps with any tolerable degree of uniformity and reciprocal similarity. If the knife be very sharp and slender pointed, encountering the bone in the passage, it may be broken ; and if not pretty well fitted for the purpose, some force and dexterity will be requisite, effectually to force it through.

In consequence of the air having access, before and behind, the bone will be the less guaranteed against exfoliation, a thing of much importance. For the access of air will give occasion to a change of temperature, and a change of temperature, to a given degree, will produce inflammation.

AMPUTATION.

M. LOUIS, } “ Actuated by such princi-
 by } ples, M. Louis practised a dou-
 S. COOPER. } ble incision, different from ei-
 ther Chelsenden’s or Alanson’s method. By
 the first wound, he cut, at the same time, both
 the integuments and the loose superficial mus-
 cles : by the second, he divided those muscles
 that are deep, and closely adherent to the
 femur. On the first deep, circular cut being
 completed, M. Louis used to remove a band
 which was placed round the limb, above the
 track of the knife. This was taken off, in or-
 der to allow the divided muscles to become
 retracted, without any impediment. He next
 cut the deep adherent muscles, on a level with
 the surface of those loose ones, which had
 been divided by the first incision, and which
 had now attained their utmost state of retrac-
 tion. *In this way, he could evidently saw the
 bone very high up, and the painful dissection of
 the skin from the muscles was avoided.* M.
 Louis was conscious, that there was *more neces-
 sity* for saving muscles than skin ; he knew,
 that when an incision is made at once down to
 the bone, the retraction of the divided mus-
 cles always left the edge of the skin projecting
 a considerable way beyond them. Hence, he
 deemed the plan of first *saving a portion of skin,*
 by dissecting it from the muscles, and turning
 it up, quite *unnecessary.* The impartial reader,
 who takes the trouble to read the remarks on

amputation, published by the greatest of the French surgeons, (Louis,) in the *Mem. de l'Acad. de Chirurg.* will at once be impressed with the force and perspicuity of the matter, and with the evident propriety of the practice inculcated."

In the instructions on amputation, given only in the preceding paragraph of his Dictionary, it is laid down as becoming and proper, that "the cellular substance connecting the skin, immediately above the wound, with the fascia, *is next to be divided all round the limb, till as much skin can be drawn back, as will afterwards, conjointly with the muscles, cut in a mode described in the foregoing account of Mr. Alanson's plan, cover the end of the stump with the utmost facility.*"*

If the propriety of the practice inculcated by Louis, be so very evident, in what manner are we to understand the language and admonition of Mr. Cooper himself? M. Louis leaves the skin attached to the superficial muscles—M. Cooper advises it to be separated, and that very considerably, for it, conjointly with the muscles, is to cover the end of the stump "*with the utmost facility.*"

If Mr. Louis's injunction is founded in the most evident propriety, what shall we admit

* S. Cooper.

as to the recommendation of Mr. Cooper, seeing that they are in contrast to each other? They both cannot be founded in propriety, or be equally the dictate of manly sense. Especially are we at a loss to comprehend Mr. Cooper where he says, that "M. Louis was conscious of there being more necessity for saving the *muscles* than *skin*;" inasmuch, as Mr. Cooper adds not a word as to the propriety or advantage of detaching the muscles from the bone, as the *best surgeons* uniformly do, in order to have the greater muscular covering. It is granted, that there is a great necessity (we will not say greater, or even so great, as that of saving skin,) for the muscular substance, and it is on the great and extensive advantage of this saving of the muscles, that we bottom the decided superiority of the method of Kirkland, and B. Bell, over those of Mr. Cooper, Alanson, Louis, or any other surgeon.

' But we have other objections against this method of M. Louis, which, in the estimation of our surgical historian, is entitled to so much consideration with the impartial reader. One objection is, the great muscles of the thigh (the vasti) do not lie stratum superstratum, but are attached to the thigh bone, nearly from one to the other, and present a great proportion of the surface on which the skin is bedded. The moment the knife passes through the integuments, it encounters the ex-

terior fasciculi of the vasti, and gets through them only when it revolves round the bone. Nor are the vasti, in any part of their substance, so constituted, as for one stratum to slide upon another. It is true, that the biceps cruris, the semi-membranosus, and tendinosus, together with the gracilis sartorius, and great adductor, may be cut through, and may retract, but still the vasti being only partially divided, would prevent all general muscular retraction.

The advantages of M. Louis's plan are imaginary, and founded wholly in an anatomical misprision as to the muscular constitution of the thigh.

Another objection to the method of M. Louis is that, notwithstanding the stump might not be conical, or sugar-loaf, in structure, the end of the bone could not be covered by any portion of muscular substance. It is only by detaching the muscles immediately adherent to the bone, for an inch or more, from the bone, and the muscles being carefully and firmly drawn up after the separation, and the bone severed close up at the surface of the retractor, that this provision can be made.

The plan of Louis merely avoids one evil. It saves from a conical stump; but it does not make provision for a muscular cushion to the bone, notwithstanding any force or power,

straps or cross stitches can afford, to keep down the integuments and muscles over the end of the bone, to which ingenuity or fancy may resort. I speak to fact, and to observation.

Moreover, even were there an adequate provision in the plan of Louis, with respect to the end of the bone, the surface of the muscles would be exposed and unprotected ; for, whether the muscles be wholly divided by one cut, or by two, the diameter of the thigh must be the same, and require that quantity of skin which may be laid over the stump, in the utmost ease and freedom. And this, in quantity, cannot be below the semi-diameter of the thigh. So that, as far as regards the exposed muscles, M. Louis left amputation as he found it—in a very imperfect condition. Whether, as has been remarked, the first circular incision be partly through the muscles, or entirely to the bone, the muscles that retract will carry with them the integuments, and there can be no covering remaining for the face of the muscles.

AMPUTATION.

<p>SAMUEL COOPER, 1809.</p>	}	<p>The tourniquet being applied as is usual, “ an assistant, firmly grasping the thigh with both hands, is to draw upwards the skin and muscle, while the surgeon makes a circular incision, as quickly as possible,</p>
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through the integuments down to the muscles. When the thigh is bulky, the large amputating knife will be found the best.—The cellular substance connecting the skin, immediately above this wound, with the fascia, is next to be divided all round the limb, till as much skin can be drawn back as will afterwards, conjointly with the muscles, cut in a mode described in the foregoing account of Mr. Alanson's plan, cover the end of the stump with the utmost facility. The detached skin is then to be turned up, in order to be out of the way, at the same time, cutting the muscles, and sawing the bone. Here, it seems useless to repeat the explanation of the division of the muscles, as practised by Mr. Alanson, and still adhered to by the *generality* of surgeons."

Mr. Cooper tells us, in his title-page, that his work is, "A Dictionary of Practical Surgery : containing a *complete* exhibition of the present state of the principles and practice of Surgery." And his chapter on amputation is headed by, "Amputation of the Thigh, as practised by the *best* modern surgeons."

Perspicacity, discrimination, and industry, in the collection of his materials, and honesty and faithfulness, in the publication, are requisites indispensable in every historian. In addition, if a historian can be so fortunate as, with intelligence, to infuse into his narra-

tive the spirit of philosophy, to accompany it with a successful and suitable enquiry into the causes and springs of action, so much the more reputable to himself, and agreeable to his reader. But inattention to the collection of materials, and to a previous examination of every accessible source, in an author, who spontaneously will assume to himself the rank and responsibility of a historian, is, in a high degree, censurable. His work is not only dishonourable to himself, but an insult to the knowledge and understanding of the world.

Were a reader, not in some degree previously acquainted with what has been written, in the various ages of the world, on surgery, to cast his eye over Mr. Cooper's Dictionary of Surgery, especially on amputation, he would conclude, that there had been great supineness, or ignorance, or both, displayed in the investigation, connected with this most important and valuable art. He would not be a little astonished to find, that after the labours and efforts of nearly two thousand years, we still lingered about the confines of barbarism.

When Mr. Cooper, as a historian, tells us, that his Dictionary is a *complete* exhibition of the present state of the principles and practice of surgery, collected from the *best* original sources of information, he surely does not intend to convey to his reader the idea, that there is no other surgery, or surgical writers,

than such as he has referred to. I mean no other good, respectable authorities. I have read much good surgery, from the writers of Edinburgh ; and there is a great deal of good sense to be found in Mynors, and Kirkland, of England. Nor do I apprehend that, upon a sober and philosophick examination of the methods of amputation recommended by Benj. Bell, and Kirkland, the reader will find nearly as much exceptionable matter, as to cause their works to be passed by in silence.

I do not feel disposed to tolerate the liberty Mr. Cooper has taken with our understanding, if he is really serious, when he considers his Dictionary to be a *complete* history of valuable surgery. Were I to give an opinion on the undertaking of this gentleman, my decision would rather be, that his Dictionary is an exhibition of what the principles and practice of, at least enlightened, rational, surgery are not.

If Mr. Cooper intends the world to understand, by his complete history, that such are the principles and practice of his Office, and a neighbouring Hospital or two, which may be disposed to adhere to old usages, I have nothing to say, because I know nothing of the transactions of those respectable sources.

But I am not content to admit, that the plan of Mr. Alanson, as good as its friends

may conceive it to be, is adhered to by the generality of surgeons of Britain. To my private knowledge, it is not adhered to in Scotland ; perhaps not practised at all by any surgeon of reputation. If the reader should wish farther authority, he may consult the valuable pages of Mr. Benjamin Bell.

The plan of Alanson, is certainly not adhered to by Kirkland, undoubtedly among the best writers on surgery, England has ever produced. And I am disposed to question, for reasons heretofore adduced, whether the method of Mr. Alanson be adhered to, in its full extent, as to principle and practice, by half a dozen surgeons in the kingdom. My reason is simple and at hand ; it requires too much skill, adroitness, and precaution, for the temper, the address, and practical professional ability, of surgeons in general. A Mr. Alanson, or a Mr. S. Cooper, may possibly get through without soiling their reputation ; but it is well known, that the common mass are too heavy of hand for such delicate and dexterous undertakings ; and it is believed, moreover, that the generality of British surgeons are men of too much sterling sense ; of too improved education, and enlightened science, to be adherents to a plan that offers so little, and is encumbered with so much, while the methods of Kirkland and Bell, are offered for their adoption. Had Mr. Cooper taken notice of the methods of Mr. Alanson, and M.

Louis, as parts of his historical narrative, and as attempts in an important art, from which their authors derived much credit, and for which they deserved commensurate commendation, I should have held in high estimation, and grateful recollection, his discretion and his prudence.

When Mr. Cooper published his history, it should be recollected that the works of Kirkland and Bell, had been before the publick eye for twenty years and upwards, and probably constituted a part of the library of almost every surgeon of London. Yet, in regard to those respectable and valuable authorities, a profound silence is observed by Mr. Cooper, in his chapter on amputation, when detailing the various methods of this interesting operation, for the instruction of the world !

AMPUTATION.

JNO. S. DORSEY, } “ The surgeon, with his
1813. } large knife, makes a straight
incision completely round
the limb, extending in depth through the skin and fat, down to the fascia femoris. This skin and fat are then to be dissected loose from the fascia, and for this purpose, the amputating knife is to be exchanged for the scalpel ; when a sufficiency of integument is thus separated, to cover the end of the stump, it is to be

reverted, and held back by an assistant. The surgeon again takes the amputating knife, and divides the muscles down to the bone, close to the reverted integuments. In this stage of the operation, he should guide the edge of the knife by his eye, and be particularly cautious not to cut through the skin turned up—a piece of awkwardness utterly inexcusable. The retractor, a piece of linen, or leather, is now applied, to *defend the muscles from the saw*, and, the assistants holding the limb above and below very steadily, the surgeon proceeds to saw through the bone. The cautions I wish to offer, on this stage of the operation, are, not to scrape up with the knife, the periosteum from the bone—a useless, painful, and tedious business, and one which will be apt to occasion exfoliation. 2. To use long strokes of the saw, which prevents the choking of the teeth, and expedites the operation. 3. Never to use a saw in which the teeth are not set wide, so as to make a groove wider than the thickness of the blade of the saw. If this be not attended to, the saw hitches in the bone, its motions are irregular, and the bone often breaks off before it is cut through. The limb being thus removed, the surgeon proceeds to tie the femoral artery with a strong ligature; he will easily see it near the bone, and probably several other large vessels will be obvious, each of which is to be drawn out by a tenaculum, and tied in succession. The tourniquet is now to be loosened, and a jet of

blood takes place from every considerable artery not yet secured ; these are all to be taken up, and the stump washed with warm water and a clean sponge. The tourniquet and compress are now to be taken off."

" The bleeding having ceased, it is proper to wait a few minutes, and give the patient a little wine to excite the circulation, as a fainty state often stops the bleeding, for a time, before the arteries are secured."

" If no blood flows, the skin is to be drawn over the end of the stump, wiped dry, and carefully approximated, and it is of *no importance*, whether this be done in a *line from side to side*, or *from before backwards*. The ligatures are to be brought out at each angle of the wound, and a *small piece of lint interposed between the edges of the skin*, in order to prevent the union of the skin, before the flap and muscles have united. A caution of much importance, as the neglect of it has occasioned, in many cases, an abscess to form, from the confinement of *the blood and serum under the flaps*, which have greatly retarded the cure. The occurrence of this accident is, I believe, a very principal reason why the French surgeons do not avail themselves of the adhesive inflammation in the present case. They believe it impossible for the skin and muscles to unite by the first intention, and they consequently cover the stump with lint. The fact is, much of the

union will be accomplished by the first intention, if the plan I have suggested be adopted. Straps of adhesive plaster are now to be applied, in order to secure the skin in its proper situation ; over this, a pledget of lint, spread with simple cerate, and over this pledget, another of fine tow, which is to be secured by a roller, &c. &c."

" The patient should be kept at rest—some pain and soreness of the thigh may be expected, but the stump should not be touched till the eighth day, in cool weather, or the fourth or fifth day, in summer, at which time it is to be dressed."

On the plan of operation recommended by Doctor Dorsey, I shall not detain the reader. In it there is little to be approved ; much to be reprehended. But it is before the publick, and if any, the least conceivable, degree of gratification, or instruction, can be derived from the perusal of the pages quoted, the author of the *Elements of Surgery*, will not have written in vain. To indicate the particular faulty points, in the chapter on amputation, of the *Elements of Surgery*, were to blot the whole.

AMPUTATION.

LARREY, } " In these cases we follow-
 by } ed the plan of Petit, namely,
 Dr. R. HALL. } after dividing the skin, and
 1814. } separating it from the mus-
 cles, as high up as is deemed necessary, we cut
 through the *superficial layer of loose muscles*, in-
 cluding the vessels, and finish the second by a
 deeper cut above the first, and, in this man-
 ner, the conical shape of the stump is pre-
 vented. I do not heal the stump by the first
 intention, (the reasons, for which, I have given
 in several parts of my work,) but approximate
 the edges with a split roller, which covers the
 whole wound, and a compress round the whole
 stump."

In the above excerption, from M. Larrey, we have a triple cut. Of the first cut, through the integuments, I decidedly approve ; but what is meant by the *superficial layer of loose muscles*, I do not exactly comprehend. The great (the vasti) muscles of the thigh arise, high up in their first fasciculi, and continue to receive, until they arrive near to the condyles, regular accessions of fibres. These muscles, at least the external one, constitute a bed for the integuments, having no superficial muscles interposed. So soon as the knife gets through the integument, and adiposa membrana, it enters the external vastus, and, if the operation be low down towards the condyles, where pro-

priety and choice will always place it, when they can be exercised, the internal vastus will also be encountered. Indeed, low down, and the thigh should be saved in its length as much as possible, the two vasti, the rectus, and the cruræus, all run into one general mass, and descend towards the patella, in complete and intimate union. Hence they have, with much propriety, been said to be a kind of quadriceps.

Nor is there any part of the thigh, from one end to the other, where a superficial layer, regularly round the limb, can be said to exist; much less can such a loose layer be referred to, as serving for a line and rule of conduct to the surgeon. The sartorius, gracilis, and hamstring muscles, with the biceps cruris, may be termed superficial and loose. But the adductors, which lay themselves along down the bone in extended lines, and two vasti, are certainly neither superficial nor loose. The integuments are in contact on one surface, and, by another, they are inserted into the bone. I am confident that no Anatomist knows any thing of this *superficial loose layer*. The same kind of unintelligible style has been used by M. Louis. Unintelligible to a surgeon, because it does not refer to anatomical facts.*

* The reader, I trust, will not consider this repetition, in substance, of what was said on Petit, as unnecessary and improper, when he considers the importance and extent of this figment of a *superficial loose layer* of muscles in the thigh, and how well

Again, What could have been the object with M. Petit, or M. Larrey, "in dividing the skin from the muscles, as high up as is deemed necessary." By dividing the skin from the muscles, nothing possibly could have been gained, except the skin were afterwards reverted, which does not appear to have been the object, for such is not expressed to have been done. The writer does not even mention, that the skin was retracted, but that, I will presume, was done. Yet, dividing the skin from the muscles, would not so much as facilitate the execution of that intention, without the forces were restricted, in their operation, to the portion of the skin thus detached from the muscles. So far as premises are furnished, from which we may judge, this separation of the skin from the subjacent muscles, was a very unnecessary, and unuseful dissection, except, at the same time, it had been reverted.

M. Larrey remarks, that he does not heal by the first intention. I should be gratified to understand what was to be united, by the first intention, in an operation where the stump was a regular plane, and even, without flaps of integuments to be laid in upon the cut muscles, or projection of muscles to cover the end of the divided bone. It appears, that the sole object was to guard against a conical stump.

suited it is, under the authorities quoted, to impose on the thoughtless credulity of inexperienced young surgeons.

A point of importance, very truly, but not the only thing entitled to the surgeon's notice.

Having proceeded thus far, in examining the schemes of other writers, I shall now detain the reader, a moment or two, while I exhibit a view of my own speculation. And as, in my examinations, no restricted liberty has been imposed, in regard to the sentiments of others, so I expect that my follies will not altogether escape the rough hand of some honest genius, who may, as I do, conceive the subjects of science to be more the objects of respect and consideration, than the eleemosynary claims of particular names or authorities. So let it be : I grant that the exuberance of folly, wherever found, and by whomsoever favoured, should be repressed—not invited out ; should be cauterized—not dressed with bland cerate. To approach rudely, personal feeling, or private reputation, is ungenerous and savage ; but, on the other hand, to let nonsense pass without censure, because it has received the oblations of eulogy, on the altar of weakness, offered by the hand of inconsideration, is to abandon the cause of science, and prostrate the dignity of independent literary privilege.

Between ten and fifteen years ago, having made up my mind to give private lectures, I engaged with much industry, and, I hope, carefulness, in an investigation of the opinions and works of the surgical writers of France

and Britain, and such others as, from circumstances, admitted of access. Being particularly anxious to bring amputation, an operation interesting, from its importance and frequency, to some degree of perfection, I, with great minuteness and attention, looked into the various authorities which treated of it. But, after much, and laboured research, had to return to the point at which I commenced ; I had again to take into consideration the method practised at Edinburgh, (a school at which I passed some years,) as that method is described by Mr. B. Bell. Yet with this, although by far superior to any other plan, whether of Britain or France, I could not be satisfied. The deformities occasioned by the angles of the circular flap, when laid in upon the face of the stump, displeased ; and, what was of more serious moment, I found them to be, not unfrequently, sources of annoyance and discommodity to the patient ; sinuous abscesses at times took place ; and the dressings could not be laid on with ease and regularity. It wanted the aspect of a finish.

While engaged on this subject, I conceived the plan of combining the methods of Bell and Kirkland. Or rather, of adopting all of Kirkland's manner, except the having the seam to go from one side, across the face of the stump, to the other. I flattered myself, that by having two semielliptical flaps, one on either side,

with the seam effected by their union on the stump, to run from above downwards, or from the front to the back part of the thigh, I should combine all possible advantages, without augmenting any one evil. The two flaps meeting smoothly and regularly in front, having the solid angles, left by Mr. Bell's plan, cut away, would form a neat and uniform stump, with an open point, above and below, for the disposition of the ligatures. The division below, should ascend rather higher up the thigh than the one above, in order that the main portion of the ligatures may be the more conveniently disposed of at this point, and that a larger outlet should be furnished, at the most depending part, for the discharge of such fluids as might be deposited between the flaps united in front, and the surface of the stump itself. This would, also, greatly favour the union by the first intention; a process of nature, of which the surgeon should never lose sight.

This method affords an opportunity for all the recently divided surfaces to be in contact with each other; whereby, without doubt, the most bland and natural dressings they possibly could be accommodated with, will be provided. No foreign body, except the ligatures, which are unavoidable, will be in any degree in the way to disturb the early operations of nature—to provoke inflammation—or disquiet the economy of the parts. And it was a pri-

mary object in my plan, that the face of the stump, when once covered by the flaps, should never afterwards be exposed. I therefore conceived the propriety of carefully removing and renewing my adhesive plasters, with which the flaps were held in their relation to the stump, for fifteen or twenty days. This general scheme, realized, is my present method. A method, I am persuaded, which unites more, and greater advantages, than any other as yet acted on.

It is the method of Kirkland, in fact, with a change of the course of the seam. Or, it is that of Mr. B. Bell, with his circular flap cut into two semi-elliptical flaps. I do not detach the muscles from the bone, as high up as Kirkland, but rather more than Bell does; I go beyond one inch, but not to two inches.

The operation, as practised by Kirkland, is highly exceptionable. The seam, going from side to side, leaves the under flap so that, if it be laid up against the face of the stump, it must ever serve as a pocket to receive the serum and blood, and, thus, ever prove to be a source of incurable evil.

The reader will readily perceive, that my claims are very limited, for the modification, I recommend, is simple in the extreme. Yet, such as it is, it changes an operation, of but secondary character, into the nearest approach

to a finished state, of any that has ever been invented. With it, as it now is, I flatter myself the reader will be pleased : I should, however, be highly gratified to see it improved ; or to see another offered, embracing superior advantages.

The operator, having laid his tourniquet as is customary, and a good cushion being placed along the course of the artery, may form the two flaps, by first making two lateral cuts with the large knife, mutually approaching, above and below, to each other, to within two inches ; and then, with an ordinary scalpel, bringing them into union in the centre of the thigh, before and behind, gradually ascending to about an inch and a half above the level of the first two lateral cuts. Or, as I have seen my able friend Dr. Thomas Wright do, make them by a dexterous management of the scalpel, first on the one side, and then the other.

These two methods I have uniformly recommended to the pupils, but have not as yet adopted the use of the scalpel. Not that I have any objection to it. I prefer it, but have not practised it, merely because I adopted first the use of the larger knife.

The flaps being shaped out, are to be dissected up from the subjacent muscles, until they are sufficient to cover, freely and easily, the whole stump when laid together. That

is, each flap must be at least the semi-diameter of the limb, and so full as not to be in any way upon the stretch when laid down. The flaps being thus dissected from the muscles, and reflected back on the upper part of the thigh, a circular cut, as recommended by Kirkland and Bell, is to be made with the large knife perpendicularly down to the bone, and completely round it. The surgeon again, when he shall have arrived at the bone, receives the small knife, and, with it, separates the muscles from the bone, to about an inch and rather more. He then lays aside the knife, has the muscles well retracted, by an assistant using the metallick retractors, or a piece of strong linen slit up the middle for some distance. The muscles being thus well drawn up, the saw is to be applied as closely as possible to the edge of the muscles, and the bone sawed off. Care should be taken, that no part of the bone which remains be denuded. For if the bone be exposed on its sides, by the periosteum being removed, it is apt to exfoliate.

The arteries are now to be secured, by the aid of the tenaculum and ligature. The needle is sometimes necessary when an artery, of such a magnitude as pours out blood anywise freely, lays itself closely amongst the bone. All the vessels that bleed, should be secured by ligature. Sometimes arteries, which do not pour out blood while the stump is exposed,

will, when the patient is put into bed, and the heart is recovered in its ordinary powers, bleed freely and become troublesome. The cause of this temporary cessation of bleeding, and of the recurrence, will be attended to, in a particular manner, in the section of this sketch which treats of the pathology of cut arteries.

The ligatures are now to be disposed along the angles, chiefly through the lower one, and the flaps well coaptated and adjusted to the face of the stump, and secured by adhesive straps, as has been remarked. In every subsequent dressing, much caution is necessary to be observed, that the ligatures may not be interfered with. And as it is a primary object, to effect all that may be possible by the first healthy process—that of the first intention, in the language of surgeons, the straps should at each dressing be raised with much circumspection, one at a time. Whenever a strap is removed, the place should be well cleaned, and another laid in its place ; care being taken to keep the edges of the lips of the flaps, in just and seemly opposition. This is to be regarded in the removal of every strap, and at each successive dressing, until every part of the wound shall be consolidated. The ligatures will, for the most part, be away, from the twelfth to the twentieth day. In the general, all the parts unoccupied by the ligatures, will be well and solid by the time the ligatures shall come away.

At the time the straps are first laid on, a layer of lint should be placed along the seam, and generally over the stump ; and over this again, a piece of soft old linen, neatly spread with any common cerate, as a general fence against disturbing causes from without, and to aid in keeping the lint in its relation to the surface, together with detaining a proper degree of warmth and moisture about the stump. The whole of the above dressing to be secured by a roller, coming spirally down the limb in even and smooth circumvolutions ; having first been passed round the body of the patient, and made fast with pins. To give the roller complete control over the dressings, the linen, covered with cerate, should rise a little way up the limb, or, which is preferable, a soft linen band should be laid on the outside of the general dressing ; ascending up the limb, on each side, five or six inches, according to the recommendation of Bell. This linen band must not be so broad as to extend to the angles ; the lower one of which must always be left pretty free, for the easy and ready escape of the exudations from within.

The plan recommended by me, I have taken the liberty to call the **AMERICAN METHOD** ; and if the reader will carefully and honestly contrast with it, either in general principle, or particular detail, any, or all of the modes pursued by the French, or practised by the British surgeons, he will, I flat-

ter myself, perceive that we are not behind France, or Britain, in any particular whatever in amputation ; but, on the contrary, that the **AMERICAN METHOD** is possessed of decided advantages.

It may be said, that the main principles are borrowed from the British. This is granted. And are not all our plans of education—and manners—and morals—and even government, itself, borrowed from the Greeks—the Romans—the French, or the British ? But, in this, we grant nothing more than that they lived before us, and are older than we are. If foreigners, or former ages, have adopted that which is good and suitable, we are not in a petulant humour to cast it away, because it has not been our good fortune first to discover it. Men, like nations, can only differ from each other in shades of character ; the general portrait and outlines must be the same, or men are no longer men.

Milton's *Paradise Lost*, in its structure, was not contrived by his own genius, but was laid out on the general principles and plan of the celebrated Greek, and other of the ancients. Yet we would not, from this fact, conclude that the Briton is devoid of all merit, and that he was indebted to Homer, and to other less poets, for his imperishable fame. No ; Milton was a thinker for himself, and he followed the general contexture of the ancient

heroick poems, only because his solid judgment, and masculine robustness of reasoning, taught him they were the best.

The operation invented by Kirkland, as he left it, offered but few advantages to the surgeon. It was assuredly inferior to that of Mr. B. Bell, but, as it is now modified, I flatter myself it places our art in an improved state.

The plan proposed, is equally suitable to the arm as to the thigh, and not inapplicable to the leg or fore-arm. In the leg, the seam can run, from a little without the shin, rather obliquely through and backwards, between the tibia and fibula. Or, indeed, from side to side, as the leg can be placed on its outer surface. When practised on the fore-arm, the joining of the flaps may be along from the radical to the ulnar edge.

It has been objected, that the formation of the flaps, recommended in the method adopted and practised by me, involves much complexity; to surmount which, no little address is requisite on the part of the operator. This objection, however, the operator on trial will find, to be formidable in imagination only.

To refer the reader wholly to my own experience would be, I confess, unfair. For he would readily recollect, that there has been no new project recommended to the world, which

has not always been successful in the hands of the projector, although it might disappoint the expectations of every body else. Some writers have discovered the art of writing *practical cases*, without practical employment ; and others, of minutely detailing operations in which they never engaged, and sometimes narrating, what they have really done, in a style which has been at variance with the facts. I must, therefore, chiefly rest the matter on its intrinsick merit ; together with the testimony of a few practitioners of character.

My respectable friend, Dr. Howard, has repeatedly assured me that, on several trials, he has encountered no serious impediment in constructing the flaps. And I have had occasion to see flaps, formed by the hand of Dr. Middleton, in which there was every thing of neatness and precision. It is true, Dr. Middleton is a gentleman of skill and address, and an excellent Anatomist.

Dr. Wright, to whom reference has already been made, in a paragraph or two above, also is in the uniform habit of operating by the double lateral flap, and speaks in the most unreserved language of the plan, as a thing of easy and ready execution. Of the competency of this judicious operator to judge, or to act, there can be but one opinion, with all who have ever seen him use the knife. Indeed, even friendship itself might be permit-

ted to indulge in the language of commendation, when speaking of this gentleman.

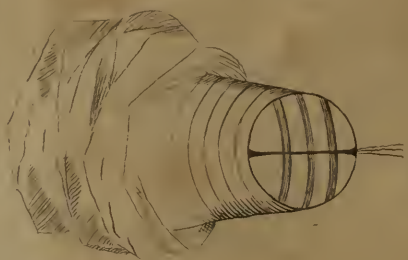
I must also avail myself of the present occasion, to refer to the flattering opinion of Dr. Wellford, of Virginia ; a physician who has always ranked among the first in his profession. To repeat his words, were to forget that I am writing on a subject nearly connected with my own private reputation.

I might also mention the authority of the very learned and able Dr. J. A. Smith, lately Professor of Anatomy and Surgery in New York—now Principal of William and Mary Academy, of Virginia. On seeing two or three stumps, formed by the plan recommended, the Professor, without hesitation, declared them to be the neatest and the best he had ever met with. He has visited the Hospitals of London and Paris.

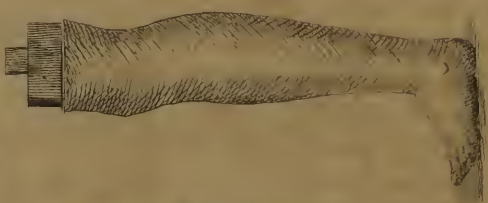
As to my own practice, I will but barely remark, that the experience of the advantages of the method, under consideration, for fifteen years, has furnished every thing to gratify—nothing to disappoint my most sanguine expectations.



V



VI



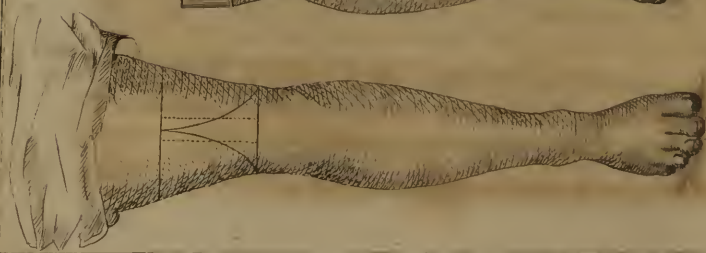
IV



III



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SECTION II.

INFLAMMATION.*

IN the animal constitution, there are not only powers of conservation, but also powers of reparation : not only those resources by which the animal is continued in life, and in enjoyment, but also such as repair the general or

* The propriety of annexing a disquisition on Inflammation, to a work professedly on Surgery, may well be doubted. Surgery, or more properly Chirurgery, (*χειρ*, a hand and *εργον* work) it is acknowledged, has no immediate connection with either the etiology or pathology of inflammation in general, although it may with inflammation under particular circumstances ; nor would a nosological enquiry, with respect to its sensible signs, necessarily involve us in surgical philosophy, or the principles of our manual operations ; nor, indeed, the more direct pretensions of its practice. Inflammation refers to disease ; surgery to the various manual methods of addressing ourselves to the removal of local affections accessible to the hand. But as the resources of our profession are not unfrequently called on, in cases of inflammation consequent on amputation ; of phlegmon, &c. the author has deemed it not improper to have the section more immediately treating of the manner of amputation, succeeded by a discussion of the elements upon which he apprehends inflammation to depend, or of which it may be constituted.

local evils to which it may, by casualty or design, be subjected. Of the former, are the powers of digestion ; circulation ; respiration ; secretion ; those by which a given temperature is sustained, &c. Of the latter, such as, when the body shall, in its general economy, be disturbed by fever or otherwise, may enable it to regain its original health ; or, when the body shall be partially disabled, by local impairment, may furnish the means of redressing the disability. Instances of this occur, where bones are broken and repaired—masses of flesh are lost, and renewed by granulation, and smoothed off by cicatrization. To these may be added more simple injuries, in which, when managed by the present improved rules of our art, a restoration of health and function is brought about, by what is surgically termed “ the healing by the first intention.”

Those are all equally processes of nature, and depend on the resources of the animal constitution. For whether we successfully attend to a general, or local malady, our science can act but an indirect part. We may give *occasion* to the removal of the disease ; we may disencumber the animal economy of evil, so far as that its own proper laws may come into operation ; but, surely, to talk of directly and positively healing the human body, is to use a language unintelligible to a rational understanding. Our claims at best, as to the process in itself, are rather negative ; and a

modest philosophy would teach us, that we are to *aid* nature—not to take matters “out of her hands,” both uncivilly and unprofitably turning her out the door.

The union, by the first process of nature, has been, by Mr. Jno. Hunter, referred to a species of action, which he has been pleased to term the action of *healthy inflammation*—Now were a man, inclined to disputation, to rise up in opposition to Mr. Hunter, and insist, that this union, by the first process, is not an act of *healthy inflammation*, but an operation of *inflammatory health*, I should be gratified to be informed, on what ground a man of ordinary sense would accommodate himself, to determine which of the two disputants were the more correct : or, indeed, how a definable line of distinction could be run between the two opinions, they so necessarily and completely lose themselves in each other, and in nonsense.

The line which divides *healthy inflammation* from *inflammatory health*, is, to my mind, not clear in its course, and approaches very nearly, in nature, to that thin partition which, it may be said, divides real insanity from affected wit. But, for a man of *uncommon* sense to be incomprehensible in his notions and terms, may perhaps be pardonable ; at least, the shadow of his name may shield him from reproach.

There can be no agencies, tending to living organization, but such as are derived from the laws of life. Whatever is of inflammation is hostile to life, and must more or less disturb its laws and economy. This proposition is defensible, or disease ceases to be disease, and health and inflammation become synonymous terms. Disease may disturb, and render organization extravagant and irregular, but can never convert aliment into living organized matter.

The term, *first intention*, is rather more a phrase of metaphysical, than physical science ; more appropriate to discussions of mind than matter ; I should therefore prefer, that the word *process* be substituted ; and when we speak of that natural union which takes place between two surfaces recently divided, and carefully adjusted together, that we say, the union by the first process of nature.

The notion, that this union is in any degree an operation of disease, is intolerably gross ; and nothing but the high authority of Mr. Jno. Hunter, could so long have countenanced the incongruity that morbid action tends to promote organized life.

When two surfaces, fresh and uninflamed, are with address applied mutually to each other, if the life of the parts has not been seriously affected, the laws of animation still in

a great degree hold dominion, and the natural process in part is sustained. Together with this, the repairing powers resident in every part, to which I have above alluded, come into operation most manifestly, from the powers intrinsick to the constitution. Thus we have two things—the one, a process by which a peculiar fluid is secreted, by whose instrumentality the surfaces may be held in contact, and to serve as a medium through which the nascent tender vessels, pullulating from the living surfaces, may elongate themselves, and reciprocally receive each other : the other, is the construction of these very vessels, growing out from either side, to constitute the intermediate living organization, under the established laws of the economy. And this union will be more or less complete, as the powers of animality may, or may not, be impaired by the offending cause. If there has been much injury of the powers of the parts, by contusion, or the presence of foreign bodies, such as, a greater or less change of temperature, &c. the union will be little, or none at all. Or, if the surfaces be not well co-aptated, the same evils will arise, and a morbid process, that is inflammation, be the consequence. Hence the great necessity of attention and care, joined to a full knowledge of the principles of the science, in the first dressings in amputation.

Vessels do not form and grow, except in a medium congenial and natural to them ; that is, under a covering furnished by the body itself. The means and attentions of art, can serve nothing more than to fence out unnatural and annoying causes, which disturb and destroy the operations going on under the control of the laws of the animal economy. Art may disencumber from ill, and guard against molestation, but surely can effect nothing, immediately or directly, in enlivened organization. Such are the principles upon which the union by the first process occurs, taught by me in the University of Maryland for several years past.

When we speak of uniting parts—and healing parts—and cicatrizing parts, we use a language rather without meaning. Nature must unite—and heal—and cicatrize ; art can only be an auxiliary. And when gentlemen talk of “ taking diseases ” general or particular, “ out of the hands of nature,” they talk of taking our language from under the rules of syntax. As already hinted, our art should be modest and assiduous, and then perhaps it will be useful.

But should the union, by the first process, not take place, either in consequence of great injury done to the powers of life, immediately by the cause producing the impairment, or of subsequent inattention, or the impossibility

of procuring aid—the parts will fall into disease, and inflammation will arise, to the phenomena and pathology of which, I now invite the attention of the reader.

In treating of inflammation, it must not be expected that I shall discourse intimately of its nature and essence. I am familiar with its obvious signs only, and therefore must restrict my discussions to what falls under the senses, and from what is sensible, proceed on inductively, as well as I may, to the conditions and changes involved, and on which the phenomena depend. The nosology of inflammation, as that of every other morbid condition, constitutes the media through which alone the disease can, with understanding be approached. It is that alone, on which a man of sound intellect can argue, and from which alone he can deduce his conclusions.

For a little time past, there has existed a kind of affected disregard to the value and importance of nosological knowledge. And it is admitted, that the minds of some men are too limited for science, or any thing rational to find admittance. But, because some minds are too dull to learn, and others too dishonest to reason, we are not to conclude that diseases, or any other branch of knowledge, are to be comprehended otherwise than through the expressions subject to the examination of our senses. To detain an understanding, at all in-

genuous and enlightened in settling the value and necessity of nosological information, the only conceivable means by which the mind apprehends the presence or degree of disease, were not respectful. A thing self-evident, is not to be argued on.

In inflammation, perhaps, the first observable expression is that of redness : to that succeed tumefaction ; pain ; increase of heat ; and augmented pulsation, rather of the parts falling into disease, than the part chiefly occupied and congested. Or probably it would be more consentaneous to observation, to assume that these nosological signs in a given and relative degree are simultaneous.

But before I enter on an examination of what is really the pathology which give occasion to those sensible expressions, it may be requisite that I say a word or two on what is *not* the pathology of inflammation.

It is altogether unnecessary to take under consideration the more ancient doctrines of viscosity of blood ; error loci ; or even, that of Hoffman and Cullen, spasm, for a long time the peculiar favourite of an unenlightened philosophy. I will at once assume, that inflammation does *not* consist in an *increased action of the part*.

An increased action of a part, must consist in a more frequent contraction and relaxation of the arteries in the part, in relation to the arteries of other parts ; or in a more vigorous contraction only of the arteries concerned, if inflammation be a thing of increased action.

Dr. Rush, in his work on the mind, lays it down as a fact, ascertained by observation, that one set of arteries may pulsate more frequently than another, in the same system, and at the same time. His words are—" It is probable the pulsations of the arteries in the brain, were *preternaturally* frequent in the brain, in the few cases in which they were *natural*, at the wrists !"*

The philological character of the above quotation is not less remarkable, than the physiological sentiment it attempts to maintain is extravagant.

Every man in anywise conversant with the anatomy of the arteries, and the circulation of the blood, will know the doctrine advocated by Dr. Rush must be physically and necessarily incorrect. He must apprehend that the column of blood, with its basis at the valves of the ventricles of the heart, extends throughout all the branches of the whole aortick system, unbroken and uninterrupted, to the

* Rush on the Mind, p. 20.

extremity of every individual artery ; and that when a wave of blood is impelled by the contraction of the left ventricle, the whole column, however ramified, must be propelled at the same instant, and in equal degree. Of consequence, the moving column must pass through each division of the arterial system at the same moment, producing what is termed the dilatation, or dyastole of the artery, in other words the pulse, and also causing a simultaneous pressure on the extremity of each branch.

This uniform and simultaneous motion is not only possible or probable, but necessary ; otherwise the column of blood were, in the main trunk and ramifications, not continuous. It must be divided or the fluid be compressible, a conception wholly absurd to every mind in anywise acquainted with the fluid termed the blood, or else the column must move as I have assumed—and the dilatation be synchronical in every division of the arterial system. As the new wave enters the aorta, the more distant points of the ramified column must be urged against the extremities of the various branches, and the arteries dilate in all their divisions at the same instant.

This being a necessary truth, and the contraction or systole of each artery being nothing but a recovery of the artery to its quiescent, from its dilated state, it necessarily

follows, that it is physically impossible for one set of arteries to pulsate more frequently than another. Thus we may conclude, that an increase of action supposed to exist in an inflamed part, cannot be referred to any possible relative frequency of action. Then if the doctrine of an increased action of inflamed parts be defensible, it must derive its importance from a more vigorous and complete contraction of the arteries concerned.

In the systole, or contraction of every artery, the walls of the artery must mutually approach each other ; must approximate more nearly the axis, or centre of the tube ; of consequence there would be less blood contained than when in the ordinary state : the parts would become more pale—less voluminous—and lower in temperature, affording the reverse of the phenomena absolutely present in an inflammation. The increase, real and apparent, of pulsation, sensible to both the patient and physician in the inflamed parts, or even in the part itself congested, cannot be referred to a more vigorous contraction of the vessels, for a more vigorous contraction would cause the walls more completely and rapidly to retire from the exploring finger. The pulsation of which the professional man speaks, is the dilation of the walls, caused by the force from the heart, and will always be different in the part, according to the incompetency of the walls of the artery of the part to resist

the impelling energy, a tergo, or the want of balance between the general and local powers.

It is remarked in a surgical work, with which the publick has lately been favoured by the pen of Baron Boyer, that "the blood flows from all parts towards the irritated point, and gets there even contrary to its usual course, i. e. by retrograding in certain vessels until it reaches the centre of inflammation." And again, "that irritation *draws* the blood from all points of the circumference to the centre of the irritated part. This afflux of blood produces the dilation and augmentation of the action of the arteries ;" &c. "that the irritation, while it *attracts* the humours, augments the action of the solids of the part inflamed in such a manner that the vital action becomes stronger in the part, and is more manifest to our eyes by the clearer development of the phenomena which characterize it. In fact, the heat, the redness, the inflammatory tension, announce an *augmentation of the vital powers*, and the organic action of the capillaries."—p. 6, *Boyer's Surgery*.

We have, in the above quotation, a striking specimen of the happy facility with which words may be fluently thrown into order, without having the remotest intelligible meaning infused into them. "That irritation *draws* the blood from all points of the circumference to the centre of the irritated part," is predi-

ated upon principles altogether gratuitous.— It would be very difficult to understand, how the irritation could draw the blood from any one point, without, at the same time, admitting that this irritation did either augment the diameters of the old vessels, or produce new arterial channels from the heart to the part irritated. Much more difficult would it be, to conceive how the blood could flow from all parts, in progression and regression ; laterally and obliquely, from the heart to the central point of irritation, through the arteries. The veins have valves.

The current from the heart to any given point of irritation could not be augmented, except new channels, as already remarked, were opened, or old ones enlarged. For whatever might be the consequence of the vessels of the part being weakened and augmented in diameter, as regards the accumulation of the fluids in, or slow progress through them, it is very certain, that no more blood could possibly flow into them than might come in the usual way and usual quantity. I have frequently heard professional men, in loose unmeaning conversation, speak of an increased determination of blood to a diseased part, but, I confess, I was not prepared to expect that any writer, in a serious and grave style, would pour out such nonsense on paper. Surely no irritation of a foot or hand, can enlarge the great arterial trunks leading to it ; or give an

additional trunk to the ordinary set. And if it cannot, how is it possible there can be an increased flux of blood to any one point, in any given time, without an increased action, from fever or exercise, in the general circulation ?

But the learned Baron assures us, that the " blood gets there even contrary to its usual course, by retrograding in certain vessels." It is understood generally, by physiologists, that the column of blood is continuous from the heart, through all the arterial ramifications, to the extremity of each artery. If this be a fact, and I believe no man will be hardy enough to deny it, by what power is an exhalant, or small ramification, to force back this column by any retrograde movement, contrary to the impelling power of the heart and great arteries ? The vessels of the part being weakened and dilated, the blood which comes on will move through them more or less slowly, according to the augmentation and reduced power of the diseased vessels, but assuredly there can be no *retrograde* action. The thing is altogether inconceivable and impossible, except where an artery is divided or lacerated. The writer says, " the blood gets there contrary to its usual course," and if his proposition be at all admitted, I have no hesitation in saying, it must be assented to contrary to the usual understanding of every man possessed of a sound intellect.

On that part of the Baron's opinion which refers the accumulation, in part, to a lateral and oblique flux, for an oblique and lateral flux there must be, if "the irritation draws the blood from all points of the circumference," the reader no doubt will permit me to be silent, his own common sense having anticipated the absurdity.

But we are farther informed, "that the *afflux* of blood produces the *dilatation* and *augmentation* of the arteries." Hence we find a very improbable consequence bottomed upon a groundless assumption. Not only is there an afflux of blood from all surrounding points, which I flatter myself has been shown to be a thing of imagination only, but this imaginary afflux produces a dilatation, and augmentation of the action, of the vessels. The author does not say mechanically or chemically, and I believe the reader will be, equally with myself, totally at a loss to conceive how any afflux could produce either a dilatation of the vessels, or an augmentation of the action of the vessels of the part.

That an irritating cause could produce a reduction of the powers of life in the vessels, and that there would be a consequential yielding in the coats of the vessels to the impetus of the ordinary current, and thus a dilatation and augmentation of the vessels take place, the reader might admit, but I am confident

he will experience no less difficulty, than that under which I labour, to comprehend how this afflux can occur, and produce this dilatation and augmentation of which the Baron so unreservedly discourses.

Again,—“ In fact,” says the author, “ the heat, the redness, the inflammatory tension, announce an *augmentation of the vital powers*, and the organick action of the capillaries.”

The fullness of vital power in every part, I apprehend, consists in the perfection of the function, under the natural laws of the part, going on, in harmony and ease, according to the purposes for which its economy is constituted—that is, in the maintainance of the most perfect health. In proportion as the function may be disturbed or interrupted, and the organization forced into disarray, must be the reduction of the powers natural to the part—that is, the powers of life. And whatever disturbs the function, or breaks up the organization of a part, must act primarily on its powers of life. For so long as the powers of life can resist, so long will the economy remain settled, and the texture be, according to the design of nature, unhurt and uninjured. There can neither be preternatural redness, nor pain, nor heat, nor inflammatory tension, without disease.

If the vital powers are augmented by inflammation, it were a discongruity in the extreme, to assume that inflammation is disease ; or could produce any condition but that of a higher gradation of health. For what is health but a given condition of vital power ? And what could be an increase of health otherwise than an augmentation of this vital power ? If an augmentation of vital power be not an augmentation of health, then have I lost all conception of the consequences of an increase of vital power and of health, and words cease to convey ideas.

If inflammation be an augmentation of the vital powers, what is to be said of gangrene and sphacelus, sometimes the consequences of inflammation ?

I shall now attempt an enquiry into the philosophy of the true pathology of the nosological signs ; and, in the first place, into that of redness.

REDNESS.

It is conceived, and correctly so, that the red colour of the animal body is ascribable altogether to the blood it may contain. And in proportion as may be the accumulation of the blood, of consequence will be the increase of redness, until the organized parts shall be, in colour, nearly or wholly that of the blood

itself. An increase of redness, in a part, is then as the augmented quantity of blood.

For any part to receive and contain an augmentation of blood, there must be a previous enlargement of capacity in the containing vessels ; the vessels are always full during health. Without an enlarged diameter of the containing, it is very obvious there cannot be an increase of the contained. Then, in inflammation, the vessel is enlarged in the ratio of its capacity to admit more blood. But, by what means are vessels enlarged in their capacities ?

Vessels may be enlarged in two ways — They may be enlarged by growth, under the natural laws of the body. But in inflammation, proportionally to its intensity, the natural laws are interrupted or suspended. It would therefore be illogical to contend, that in inflammation the diameters of the vessels are augmented by a natural process. Not only so, but much time would be required for vessels to enlarge by natural growth. Mr. Jno. Hunter, writing on this subject, remarks, “ we must suppose its action in the parts, to produce an increase of size to answer particular purposes ; and this I should call the action of dilatation, as we see the uterus increase in size in the time of uterine gestation, as well as the os tinæ in the time of labour ” &c.

Page 9 and 10, On the Blood.

Every action, in parts to answer particular purposes, must be such as are under the regulation of the natural and healthy economy of the parts ; for all particular purposes of the animal body, must be purposes which accord with its health and physical prosperity. To suppose a provision in certain action in parts of the body, for other purposes than those within the economy, and for the benefit of the body, were absurd.

The increase of size in the uterus during the time of gestation, and evolution of the os tincæ in the time of labour, are operations of nature ; effected by the laws of the parts ; necessary to important purposes ; and, without which, the scheme of nature, as regards man, would fall into immediate ruin ; and the human race be determined and ended.

If we are to attach any signification to the extract from Mr. Hunter, it must be, that inflammation is equally within the laws of the healthy economy of the body, and equally necessary to answer particular purposes of the animal system, as pregnancy and child-birth. He has referred us to the action in the uterus and the os tincæ, as an illustration by analogy of the principles of his proposition.

Mr. Jno. Hunter may, in his own estimation, have comprehended how, and to what extent, the laws of disease, which destroy the

life, and break up the organization of living parts, and the laws necessary to the healthy purposes of the animal economy, may be mutually illustrative of each other, but I confess my understanding to be wholly inadequate to trace out the parallel.

Nor can I so far agree with Mr. Hunter, in speaking of the action of the uterus during uterine gestation, or that of the os tinæ in time of labour, as to ascribe them to simple dilatation. I am of the opinion, that the philosophy is not exceptionable which refers the former to an absolute growth ; a real increase of parts by the accession of additional organization, and the latter to an operation equally natural. To speak of those actions, as mere acts of dilatation, is to indulge in a style less philosophical, and less appropriate, than mechanical and incongruous.

As it is inadmissible, that this acknowledged increase of capacity in the vessels inflamed, should be considered as ascribable to natural growth, or to an "action of dilatation," founded on a provision in the part for particular purposes, may we not consider it as resulting from a condition of weakness, superinduced by some agency unfriendly and injurious to the life of the part, and of consequence adequate to a reduction of the tone of the coats of the vessels, which tone is dependant on the living energy of the part ? The life in the

part being lowered, the coats of the vessels become less able to resist the impulsion of the circulating mass urged on by the heart. The consequence will be an enlargement of the diameter ; a less vigorous and complete recovery of the artery—and a necessary accumulation of blood in the vessels affected.

This pathology, in a comprehensible and rational manner, explains to our understanding the nosological sign of redness.

That the vessels are enlarged in their lumen, there can be no doubt—and that this enlargement is morbid is a necessary fact, otherwise inflammation is not disease, but health, and we are permitted to speculate at large in the unintelligible nonsense of *healthy inflammation*, and *inflammatory health*, unrestrained by sober science, and beyond the admonitions of common prudence.

To the opinion of Mr. Jno. Hunter, that there is an increase of size in the inflamed vessels, I will add the admission of Dr. Fowler, by far the most learned and able antagonist that ever advocated the indefensible doctrine of increased action of inflamed vessels.

This enlightened man, in his respectable Thesis, admits “ it to be moreover very probable that the redness, at least in a great degree, is owing to the colourless arteries being

distended with red blood."* Although he uses the words "*deberi vasis lymphaticis*," it is very certain he would not contend that the absorbents, or lymphaticks, strictly so called, can by any means, except by extravasation, be either turgid or even filled with red blood. He obviously, and necessarily means, the colourless arteries in which the inflammation must always commence. And by himself we have explained how it is, that these naturally colourless vessels become so as to admit the red blood. He asserts, "that the arteries, when inflamed, are weaker than when in a healthy state."† And thus it is, that a grosser fluid permeates them. The reader will recollect the condition of the colourless vessels of the tunica adnata of the eye.

The admission of the fact is all that is required. The conclusion is inevitable and necessary. If the vessels be so affected by inflammation as to be lowered in their tone, and become turgid from the accumulation of red blood, it is clear that the phenomenon of redness is, by Fowler himself, demonstrated to be consecutive of weakness. It is perfectly immaterial, in the discussion of the point before us, whether this weakness be positive as to the vessels themselves, or whether it be

* "*Valde porro probabile videtur, magna saltem ex parte, deberi vasis lymphaticis sanguine jam turgidis.*"—Fowler.

† "*Arterias inflammatas, quam in statu sano debiliores esse.*"
Fowler.

relative as to the general force of the heart and great arteries. But I would contend, with Dr. Fowler, that the vessels are absolutely weaker during inflammation than in health.

In addition to the admissions on the part of Hunter and Fowler, I might avail myself of the general fact that, when parts have been inflamed, and in a short time thereafter have been operated on, the surgeon has had to tie more vessels than when he has operated on the same parts under different circumstances. He has had to tie more, because the smaller vessels, such as, under healthy circumstances, do not admit a current of red blood, are enfeebled and enlarged in their diameters.

This opinion, that the condition of inflamed vessels is that of weakness, was probably first suggested by Dr. Kirkland, in his valuable work on medical surgery,* and was afterwards advocated, and thrown into the form of a general principle, by Mr. Allen, and Dr. Lubbock. The opinions of Mr. Allen, and Dr. Lubbock, have been referred to by Dr. Wilson, in his excellent publication on febrile diseases, who, I believe, is the first writer that has appeared before the publick, in defence of the doctrine for which I now contend, in its principle, and which, I persuade myself, only

* "We shall presently mention several instances of inflammation, where it is manifest the motion of the blood, and the action of the vessels, must be diminished."—Kirkland, p. 252.

requires to be understood, to be admitted and universally received.

The weakness of the vessels inflamed may also be argued, from the circumstance of the exhalents pouring out more, and grosser fluid, into the cellular substance, than they are accustomed to pour out ; and which becomes one source of the tumefaction. Also, the different shades of redness observable at the base and apex of a circumscribed inflammation. That at the base being of a more vivid, or arterial colour, while that of the apex is darker and venous. The more vivid we would explain, by a more immediate connexion with, and supply from the general circulation ; the darker, to the circumstance of its being in a great degree, as it were, cut off from the circulation, and becoming quiescent—the vessels in which it is contained having lost, in a great degree, their ability to contract and urge on their contents.

In page 9, Mr. Hunter again remarks that, “ This increase of red appears to arise from two causes ; the first is a dilatation of the vessels, &c. ; the second is owing, probably, to *new vessels being set up in the extravasated, uniting agglutinating lymph.*”

In the opinion of men of professional observation, there is no inflammation, produced by whatever cause, in which the red colour is

more complete, than in such as are produced by heated bodies, and such as are erysipematous. Those inflammations assume the red colour in a few minutes, or hours at least. Would Mr. Hunter contend, that new vessels set up for themselves in the space of a few hours ; have a circulation of their own ; hearts proper to themselves ; can connect themselves with the old circulating vessels, by instinct, or by thought, or by good fortune !!

That red blood can be made otherwise than by circulation ; that a circulation can be kept up in these young factories, without a heart, I would deny ; and I would further contend, that medical record, faithful and admissible, does not furnish a solitary instance of effused lymph producing blood-vessels.

When the inflammation shall have ceased, and the parts been restored to their original healthy condition, what becomes of those new factories of organization ; those new establishments of blood-vessels ? Do the greedy absorbents feed upon, and consume them to the last vestige, so that not a morsel is to be found, by the most diligent search of the keenest sense, whereby to regale the delicate palate of a fastidious philosophy ? Some men, trusting to the easy credulity of their readers, must certainly consider themselves privileged to write any thing, provided it be above or *below* the understandings of others.

Doctor Caldwell, who by a series of experiments has sedulously laboured to fortify the conclusions of Mr. Jno. Hunter, as to the *intrinsic* vitality of the blood, informs us that, “in the summer of the year 1800, he was obliged to submit to the extraction of a tooth from his lower jaw. The hemorrhage from the lacerated vessels, though not very copious, proved tedious and troublesome. Various expedients were devised to suppress it, without effect. It at length occurred to him, that if the blood were allowed to fill up the alveolus, and retained there, by pressure, till coagulation should take place, the thrombus would effectually close the mouths of the divided vessels. The experiment was made, and succeeded to his wishes. The hemorrhage, which had continued nearly three days and nights, was now at an end; and as the coagulated blood proved neither inconvenient nor disagreeable, it was suffered to remain in the socket of the jaw. The weather being warm, he examined it carefully, several times a-day, lest it might become putrid and offensive.—But instead of this, he observed it, on the fourth or fifth day after coagulation, beginning to assume the *appearance of flesh*. Nor did this incarnation *commence at the circumference*, but in the *centre of the coagulum*, at the *greatest possible distance from any vessels that might, by elongation, have been protruded from the adjacent gums*. From this central point the process continued to extend, till what had been at

first nothing but congealed blood, became a piece of perfect flesh, similar in texture and appearance to that of the gums."

The above *asserted* fact is bottomed, I presume, upon the broad basis of human *credulity*. It is entitled to our attention for many reasons.

It is, in the first place, *solitary* of its kind. Similar occurrences are not to be met with in the records of human experience ; although the Experimenter, at the close of his paragraph remarks, that " he suspects this to be a very frequent occurrence after the extraction of teeth." But I am not disposed to permit an Experimenter to reason from what he suspects may have been ; he must be restricted to what has been, and has received, moreover, the support of observation.

It is solitary of its kind, inasmuch, as in the space of from three to five days, it emitted nothing offensive ; it did not, even upon its surface, become putrid. This is the *only instance* of a clot of blood remaining in the mouth, for four or five days, that did not become in some measure offensive, and, I am pretty confident, will never be deprived of its claim by any parallel, except it be in the vigilant and careful observations and experience of the gentleman who is the subject of the present case.

Again, it is singular of its kind from its *transparency*. All other clots of blood have been observed to be opaque, so far as experience has extended, or will extend. The Experimenter does not assert it to be *transparent*, it is true. But its transparency is a thing of necessary consequence. If it were not *transparent*, in what manner did his eye, of itself, or aided by glasses, discover that the *incarnation commenced at the centre, from the greatest possible distance from the circumference?* And, “*that the process regularly extended from the central point*” to the surface. The Experimenter does not claim the dexterity of injecting and dissecting this vascular organized plug, which resembled a piece of flesh engrafted.

I again assert, without the least fear of being contradicted, that this clot must have been transparent, or the Experimenter could not have discovered what was going on in its centre, any more than I could discover what operations may be going on in the centre of the moon. As the incarnation commenced in the centre, it could not have fairly and successfully been made a subject of injection and dissection, even if hands, light and careful enough, could have been found to engage in the experiment. Not only must it have been *transparent* from the superior surface to the centre, but also from the centre to the bottom of the alveolus, otherwise some sly vagrant vessels might have unperceived crept along

towards the centre from below, and even a vigilant eye may have been deceived. But the Experimenter asserts the central incarnation to be a "*fact*" !!

I hope that I shall not be too harshly censured for my curiosity in this singular affair. I do not know how it is, but so it is, that I have such a bend to infidelity and scepticism, I cannot prevail on myself to admit speculations to be uniformly without misprision, merely because they are curious and amusing, although they serve *particular purposes*, except when they come within the range of feasibility. Such is my uncourteous nature, and so slow am I of belief.

That blood has coagulated in the alveoli of the jaws, after the extraction of teeth, is consonant to the experience of most men ; and that this blood, in the course of a few days, has been dissolved and wasted away, granulations shooting up from the surrounding and inferior surfaces, and occupying its place, is what daily observations countenance ; but that organization or incarnation has commenced in, and proceeded from the centre of such coagulation, is that on which modest experience is desirous of being silent, at least until it explains the way in which human vision, aided or unaided, can have access to the centre of opaque matter.

The reader will understand, that the clot was not extracted and dissected by the learned Experimenter.

Doctor Dorsey, in his Elements of Surgery, admonishes the reader, "On this subject" the proximate cause of inflammation, "to see Hunter on the blood, &c. And also Dr. Wilson's Essays on Febrile Diseases, in which the doctrines of Mr. Hunter are ingeniously, though I think" says the Doctor, "unsuccessfully opposed."

In this admonition we have an *opinion*. But how much disposed soever I may be to respect the *opinion* of the learned author, I cannot help recollecting, that in the estimation of all logicians, the distance between an *opinion* and an *argument* is immeasurable ; and that a gentleman who attempts to settle a point in science, by telling us he "thinks" his opponent to be wrong, occupies a space in philosophical contest, which can neither be conceived nor measured.

But, by way of securing regard to an *opinion* in a subsequent, we are furnished with a *probability* in a previous note.

"The heat and redness, are *probably* owing to the increased rapidity of the circulation ; but in what particular manner, is not clearly understood."—Page 4, vol. I.

It has already been shewn that an increase of circulation, dependent on a more frequent action of the particular blood-vessels, cannot possibly take place. And I hope with equal certainty it appears, that if the blood passed with greater rapidity through any set of vessels, from a more complete and forcible contraction of these vessels, the part must become less red, if the redness arise from the presence of blood. Hence we are not astonished to find some difficulty experienced, as to the manner in which the increase of redness could occur from the blood going more rapidly through, or from increased action of the part.

To find gentlemen not clearly understanding their own hypothesis, and to be disposed to resolve them into the form of opinions, inspires me with some hopes that they will give them up ; and that those resting upon higher degrees of probability, and to be more clearly understood, by which the phenomenon can be satisfactorily solved, will be embraced.

TUMEFACATION.

The swelling in inflammation is deducible from two sources. The enlargement of the vessels must constitute one. For if a vast multitude of vessels be enlarged, as this increase of volume must require increase of space, the general augmentation of bulk must, in part, be referrible to this vascular change.

The other source of swelling, is the effusion of the more thin and white parts of the blood into the cellular substance. But were the whole tumefaction attributable to this effusion, the colour would be that of the effused matter, that is white ; an inflammation on the contrary is red. There never is any red blood effused from the exhalants in common inflammation. And, perhaps, whenever the red part of the blood is poured out into the cellular substance, there is uniformly an injury to the integrity of the vessel.

HEAT.

From a very early period, particularly from the days of Celsus, an increase of heat has been considered as inseparable from inflammation. It is from this, in fact, that the technical term, inflammation, itself derives its origin.

To comprehend each other, it would be proper that we should explain what it is we understand, by an increase of heat in inflammation.

When a surgeon, or any man of common sense, speaks of an increase of heat in a part, he uniformly and necessarily intends to convey the idea of a contrast between the state of increase, and the natural or ordinary state. Or

he means the relative condition of the part spoken of, and that of the circumjacent parts.

To say that the heat of the part inflamed is never above the heat of the blood at the source of circulation, is, as regards the question of increased heat, to say nothing, because it is wide of the question.

“The heat of an inflamed part, never transcends the heat of the animal at the source of circulation, though the sensation would lead to a contrary belief,” says the author of the *Elements of Surgery*.

When we use the language of a science, we must understand it according to the established usage of the science, it otherwise is indeterminate. And I again repeat, that the general surgical or professional acceptance of the term “an increase of heat,” bears relation to the previous ordinary and healthy condition of the part, or the present condition of the circumjacent parts, and not to the state of heat at the source of circulation.

But admitting that it does bear relation to the last, who has ever undertaken to ascertain by “sensation,” the touch, what is the degree of heat at the source of circulation? Is this ascertainable by sensation? I apprehend not. It is a thing of thermometrical experiment, and our sensation can have nothing to do with

it. It has been already determined pretty well, by experiment with the thermometer, that the heat in fever, and also in inflammation, does at times ascend four or six degrees above the natural and healthy standard at the source of circulation.*

When a part is inflamed, like the surface in general fever, there is a disability with respect to the animal function of secretion ; of consequence the ordinary chemical means, the escape of fluid, is lost to the part, and thus there may be, and I apprehend is, in many instances, an accumulation of heat above ninety-eight, the usual temperature at the heart.—Every animal has the power of producing and regulating its own temperature, and this by laws proper to animality. Were it by chemical means, the animal heat would rise and fall according to the medium in which the animal might be.

Whenever there is a change of temperature in a part, it must be primarily ascribable to an altered animal condition. That the increase of heat is owing to increased rapidity of the circulation, cannot be correct. If the increase of circulation in the general system be alluded to—if there be no fever, the temperature remains the same as in ordinary—if there be a fever, it may be above it. A correct observation does not admit any increased rapidity of

* See Fordyce.

circulation in an inflamed part, for reasons already advanced.

Whatever may be the immediate cause of animal heat, is, as respects the question under consideration, of little moment. It is pretty well ascertained, that in proportion to the quantity of blood in a part, will be the temperature of the part. Hence we find, as the blood accumulates the heat rises, until the parts shall, to a given degree, have lost their powers of life, and, as the powers of life shall have fallen off, the temperature is depressed.

PAIN.

Another concomitant of inflammation is pain. It has already been assumed that there is effusion, and that the vessels are enlarged, if we add to those two conditions the fact, that as the tone of every part is reduced within a given degree, the sensibility and excitability are increased, we shall perhaps have some clear understanding of the cause of pain. The nerves of the parts being increased in their sensitive capacities, will but impatiently sustain the evil to which they are exposed by mechanical distraction. They are put upon the stretch by the swelling, which, added to their increased liability to be acted on from the reduction of their living energy, we conceive to be a source quite adequate to the pro-

duction of the painful sensations which accompany inflammation.

What Mr. Hunter means by a spasm in a vessel weakened and inflamed, I confess I do not comprehend. Nor does he so clearly express himself, as for us to understand whether he alludes to a spasm of the vessels, or the nerves. I know nothing of a spasm of either.

INCREASED PULSATION.

There is in and about the inflamed part an obvious increase of pulsation, discoverable both by the patient and the physician. It has already been assumed that, in every part where the tone or energy has been reduced, by whatever cause, the excitability, or susceptibility of impression, is augmented. I will except cases of palsy. This being conceded, and I believe it will not be denied, the customary force of pulsation would, in the sensitive part, and to the patient, make an impression above what was natural. But how is it, that this pulsation is really greater to the touch (the *tactus eruditus*) of the physician, if the coats of the vessels be weakened in inflammation?

In an hydraulick machine, were a wave of fluid impelled along through a set of elastick tubes, with a dilating force of forty degrees, against the walls of the tubes resisting with

the power of thirty, the impression made by the passing wave would be equal in every part of the tubes : But, if the capacity of resistance in the walls of the vessels were, in any part, only twenty, would not the dilating power in that part, meeting with less resistance, impress the hand applied on the outside with greater force ? It undoubtedly would. Apply this to the human body, and it will be readily perceived why the finger of the physician discovers a stronger action in the arteries about an inflamed part, than in arteries exactly healthy. The increase of action cannot be in the artery, for the artery by its systolic action, the only one of which it is capable, retires from the finger, and this with a rapidity in the ratio of the contracting power. The impression on the finger, is from the force of dilatation—the impelling force from the heart. And, to my understanding, there can be no other adequate reason why the passing wave should be more sensibly felt in any one particular part, than in the general system, except that of weakness, or reduced competency to resist, in the coats of the affected vessels of the part.

There may be something attributed also to a species of vibration, occasioned by the impediments met with by the column, from the congestion and effusion in the part. Dr. Wilson has taken a different view of this part of

the subject, but rather gratuitously, and upon the begged question in my apprehension.

From what has been advanced I deduce the conclusion, that the condition of inflamed vessels is that of reduced power, or relative weakness ; that this condition alone, is adequate to a solution of the phenomena of inflammation ; and that in this relative weakness, relative as to the general circulating powers, or this altered local excitability, consists the disease. Whatever is consequential, must be viewed as so many phenomena, or sensible signs of this morbid condition, and whatever precedes, as so many causes giving origin to it.

The proximate cause, in this and every other disease, I conceive to be imaginary and unreal ; not a physical thing. And every author, who has attempted to establish and explain the reality of a proximate cause, has written unintelligibly to himself, and absurdly to the understanding of his reader, from the days of Gaubius to the present time.

Whatever so far efficiently affects the powers of life, and the order of the economy in the part diseased, as to produce indirectly the phenomena of inflammation, may be said to give occasion to this morbid excitability. For on this morbid excitability depend the phenomena. As to the *manner* in which heat, or any other cause acts, in reducing the powers

of life, I know nothing ; it is wholly inscrutable. But the causation, or manner, can only be a rule of action, and not a cause.

It is, however, unnecessary to detain the reader in any discussion on the doctrine of causes, the subject being rather irrelative, and without the scope of my design—he may consult Gregory and Lieutaud.

To fortify myself still more securely on the ground a prudent philosophy has admonished me to occupy, I shall illustrate what has been advocated, by a few appeals to the ordinary usages of practical men.

When it becomes the object with the surgeon, to resolve or discuss an inflammation, his primary consideration is the reduction of the circulating forces, by abstractions of blood, catharticks, &c. The next, the removal of what, in the part inflamed, is stimulating, and also what is within the vessels, which may continue mechanically to press upon their enfeebled walls, by the topical applications of such materials as possess a large capacity for heat ; such as enable the walls to gather up, and reduce the diameter of the vessels, perhaps by chemically taking off something.—That is, he brings in to his aid articles of low temperature, and also astringents. He uses local blood-letting, which, by taking the blood

immediately from the weakened vessels, permits them to recover their former lumen.

These measures will not only tend to restore the vessels to their healthy capacities, but, by so doing, arrest the effusion, into the cellular substance, of the thinner parts of the blood. To these I might superadd the means resorted to, when the inflammation shall have been pretty well discussed, leaving something of the tumefaction, together with more or less of a discoloration of the surface, from the vessels being unable to recover their original diameters.

In those cases the surgeon resorts to gentle friction, with or without some medicated auxiliary* holding dominion over the absorbents ;

* The medical material here particularly alluded to, is mercury, by far the most powerful instrument, in the hands of instructed art, for the management of inflammation, when used as an auxiliary to the lancet.

It would be to omit doing justice to learning and merit, were I to pass by the present occasion of acquainting the reader with the manner in which, and the person by whom, mercury has been brought into general use, with respect to the treatment of inflammation.

Between one hundred and fifty, or two hundred years ago, mercury was used by the learned Morton, and by Desault, the physician, as a remedy, nearly infallible, in the resolution of Tubercles of the lungs. And Desault remarks, that it was of important use, if laid, in the form of ointment, on the region of the liver, in the removal of chronick swellings of this organ.

About seventeen hundred and eighty-nine, the use of mercury, in chronick hepatick affections, was revived by some surgeons of the East Indies, and the information of the revival, by the means

or to bandages, which are in an especial degree useful, in mechanically restoring strength to the weakened blood-vessels ; lessening pain ; and enabling the absorbents, by recovered strength, to take up what may be effused.

The reader will perceive, by a careful examination of the effects of the means of cure,

of fugitive Essays, sent to Britain, and from that, reaching this Country, became a subject of professional enquiry in this State.

Doctor George Brown, an able and sagacious physician of this city, was among the first to embark on this new current of speculation, and not only applied the article to the management of chronick affections, but extended its use to what is generally called active inflammation, especially to pneumonia, and in this, about 1790, he was said to be very successful.

This new, and as it was, ignorantly, conceived to be dangerous practice, was by some professional men, of high respectability, severely and keenly reprobated. But, it appears, from facts within my own knowledge, that the Doctor, with a firmness honourable to his masculine understanding, persisted confidently to pursue his own views, and has laid the medical publick under more heavy obligations, as regards the dominion we now hold over inflammation by mercury, as an auxiliary to the lancet, than any other man living or dead. The valuable suggestion of this judicious practitioner, has tended very much to enlarge the sphere of our professional hopes, and still, under the experience of every day, gains upon our confidence. Whatever may be due to others, for the revival of the use of mercury in chronick affections, Dr. Brown is entitled to the acknowledgments of the profession, for its extension to active inflammation. And although mercurials, under the character of *alteratives*, as they are termed, have been used in preparations for Small Pox, and prescribed in various gradations of fever, yet, so far as my recollection extends, mercury was not, until the epoch of which I am speaking, particularly addressed to the removal of inflammation, in co-operation with aperients and appropriate abstractions of blood.

Although I use the term *active inflammation*, I do not hold myself responsible for its propriety : it is certainly unscientific. But it is sometimes proper to comply with custom in the use of language.

that we may most confidently believe ourselves justified in the opinion, that weakness is the necessary condition of every inflamed part.

But, should it be judged advisable to bring on digestion or suppuration, the surgeon for a time co-operates with disease. He so orders his plan, as completely to break down the walls of the vessels, if it be a circumscribed inflammation ; or, if it be that about a stump, so as to produce an effectual relaxation of the mouths of the vessels, and an effusion of their contents. These objects he effects by moisture and heat ; by poultices and fomentations — These prepare for suppuration which is a healthy process.

If the inflammation be circumscribed and considerable, the central part is killed, and is removed in the form of matter—blood—cellular substance—and decaying blood-vessels : It is termed by surgeons the *core*.

Now it is that the *second process* of nature, the process of granulation, takes place. And in this, as in the first process, there is furnished, by a provision of nature, a congenial fluid for the new granulations to form in ; and, as a protection to them against foreign annoyance. A granulation cannot grow, or remain healthy, except enveloped in this natural secretion—pus. The measures of the surgeon can only act mediately ; they can only fence

out evil, and afford opportunity to nature to carry on her own work, which she does by laws fixed and regular, yet inscrutable. And, finally, all is smoothed off by the *third process*—cicatrization.

Thus the reader perceives the *three processes*—the first, where the powers of the part have been but little injured, and a union takes place without the phenomena of inflammation occurring—the second, that of granulation—and the third, that of cicatrization : all equally operations regulated by the laws of the constitution of the animal body, and, in which, art can perform but a secondary part.

SECTION III.

DIVIDED ARTERIES.

AFTER patients have been operated on, and reposed in bed, not unfrequently hemorrhages occur from the face of the stump. For, notwithstanding the utmost care, and strictest examination at the time of the operation, on the part of the surgeon, small vessels, of sufficient size to pour out red blood, will escape notice, either from being mechanically obstructed by repressive coagula, externally pressing on their mouths ; or the exhaustion from loss of blood or alarm, and which, when the patient recovers his powers of body, or the coagula, being slightly attached to the mouths of the vessels, are forced away, will bleed afresh. These occurrences have brought into practice the most reprehensible measures : such as the application of lint on the face of the stump ; the dusting it with flour, or powdered resin ; or

the application of sponge, agaric, blue vitriol, &c. &c. to individual vessels. All of which interfere most seriously with every part of the union by the first process, and of necessity provoke inflammation, not only by such rude management, but by exposing the surface of the stump to *unequal temperature*, the only adequate cause of inflammation, from the access of air to recently divided surfaces, or internal cavities. This opinion, of an *unequal temperature*,* the only rational one that has ever been offered to the publick, being the reason why atmospherick air is hurtful to internal cavities, was some years ago published by my late able colleague, Professor James Cocke, in an excellent and ingenious inaugural Thesis.

To avoid the evil of subsequent hemorrhage the surgeon should believe, “that every operation which is well enough, is soon enough done,” and should wait some time after the larger arteries are secured, endeavouring, by careful and gentle sponging with warm water, to effect the removal of every coagulum from the face of the stump, that the mouths of the vessels may be free and unmolested ; and the judicious exhibition to the patient of appropriate cordials, such as wine, or brandy and water. But, as to the strength of the cordials,

* The air is said to be unequal in temperature, as regards the part concerned, when, by an elevated, it produces indirect, or, by a depressed temperature, it gives occasion to direct debility, and, its consequence, augmented excitability.

the surgeon should be cautious, lest they dispose the body to fever, and the stump to inflammation.

There is nothing against which a surgeon is to be more on his guard than the old absurdities of Petit, revived by some modern writers, of coagula forming within living vessels and becoming organized, and arresting hemorrhage ; or, the equally indefensible notion, of the *contraction* of the mouths of divided arteries, formerly advocated by Kirkland.

That a divided artery, not mechanically pressed on by any circumjacent material, will contract, I hold not only to be improbable, but impossible.—The reader will consult what has been said on this subject against the speculations of Dr. Jones. in the first volume of my physical sketches, and if he can so far abandon all regard to his own common sense, as to admit the conclusions from Dr. Jones's own premises, I believe I need not attempt to reason with him. If he can believe, that a large quantity of coagulable lymph and red blood can be circumfused about the end of an artery, and ascend high up along its walls, breaking up all the cellular connection, without compressing the vessel itself, he must have a faculty of faith for which he cannot be envied.

But farther ; it is admitted by Hunter, by Fowler, by Wilson, and must be conceded by

every professional man of common observation, that the blood vessels, in every inflammation, are enlarged or increased in size.—And can it be conceived to be possible, that the small branches of vessels shall be divided without the vasa vasorum, which enter into their constitution, being irritated and inflamed? And if inflamed, a thing, necessary, then weakened; and if the cut vessel be weakened in the circle constituting the extremity—so far from contracting, the mouth must dilate.

Either the vessels, in inflammation, are not reduced in tone and enlarged in diameter, or the mouth of each cut artery must dilate.—There is no surgeon, I believe, of the most common reputation, who would say that an inflamed vessel is not enlarged. And, this being conceded, gentlemen may prove as they can, how an increase of volume, under morbid action, can take place without reduction of tone, which must be done before any man, of modest and rational pretensions, will permit himself to talk of the contraction of divided arteries. First let it be demonstrated, or at least rendered probable, that the vasa vasorum, at the edge of the circle of the cut vessels, are not irritated, inflamed, and depressed in their life, and then a more patient ear will be held to the doctrine of contraction.

Were the tourniquet to be left on the upper part of a limb, and the larger arteries not

secured by ligature, would they in any degree contract at their divided points ? They assuredly would not. Then are not the smaller arteries possessed of a similar economy, and under similar laws with the larger ? And if the larger rather dilate, the contrary of which no man can prove, upon what principles are we to expect the smaller to contract ?

As to the doctrine of coagula within a living tube, becoming organized from inherent vitality, and constituting a part of the living whole, it is to be viewed as a puerile hypothesis. The column of blood extends from the valves of the aorta to the extremity of each vessel, of consequence, upon every new wave being impelled forward, that portion which occupied the extremity must be displaced and succeeded by a fresh one ; which, on a succeeding wave, must, in its turn, be subjected to the same changes. Hence, except the mouth of the vessel be closed by some extraneous agency, time can never be afforded to the internal coagula to become organized, even if they possessed an adequate set of powers. Living organization cannot be the work of a moment, and the heart, in the general, contracts at least sixty times in a minute. Thus the organization of coagula within living vessels is, obviously, an absurdity, let the experiments of Dr. Jones prove what they may.

The coagulation without, and the coagulation with the subsequent organization, within, the intermediate effusion of coagulable lymph, and the spontaneous contraction of the cut extremities of the vessels, in the experiments of Dr. Jones, go on as temperately and smoothly, as if they had been dreamed by an experimenter asleep in his library, or had been regulated by compact !

The coagula found in the heart frequently after death, and considered by some as polypi, are the result of the cessation of life, and the ultimate contractions of the heart : They are not living masses.

From what has been said, it would appear that the hemorrhage from an artery does not cease in consequence of a spontaneous contraction of the mouth of the divided artery, nor in consequence of internal coagulation, therefore the permanent cessation of the hemorrhage must of necessity be attributable to external coagulation mechanically forcing the arterial walls into mutual contact, and this can only be done where the circulation is feeble ; or to the means, that of compress or ligature, made use of by the surgeon.

PHENOMENA OF ANIMATION.

The objects with which the human understanding may be said to be chiefly conversant, are more the qualities and accidents than the nature of things. When speaking, therefore, of life, as in disquisitions on mind or matter, I must be understood as referring to its phenomena or sensible signs, rather than to what it essentially or positively may be. The essence, or absolute nature of life, is that of which we can have but an equivocal and remote knowledge ; a knowledge by induction only. I do not permit myself to enter on a scrutiny into so recondite and abstruse a subject. It is into an enquiry on the *palpable* and obvious attributes alone of life, that an intellect, at all aware of its undertaking, can be occupied — We must be satisfied with the aids of inductive logick. Therefore, having premised a few remarks on certain opinions, I shall enter

somewhat in detail on the leading, or distinctive phenomena of life, which I conceive to be,

- | | |
|-----------------|--------------------|
| 1 Propagation, | 5 Muscular motion, |
| 2 Growth, | 6 Sleep, |
| 3 Secretion, | 7 Vigilance. |
| 4 Excitability, | |

PROPAGATION AND GROWTH.

That the first living state of man was the perfect condition of the Adult, is a proposition clear and demonstrable, and from which the mind cannot dissent. It is a necessary truth, to the authority of which, as an axiom, we are compelled to yield assent. Or, otherwise, the heavy necessity is imposed on us, to admit an animal body to be ; to acquire an increase of volume ; to possess organs ; and perform functions, without being furnished with instruments or powers commensurate to such ends.

Between this conclusion, and the proposition advanced, there is no midway. For if man were originally in the condition of an Infant, the question immediately and forcibly presents itself—how or where did he procure aliment for sustentation and growth ? Or how shelter himself from the effects of destructive agencies ? The objector may accommodate himself on the horn of the dilemma if he pleases, but with me the proposition is evi-

dent, that man must have existed, and that necessarily, at the commencement of life, in his state of majority. Thus we are, by philosophy and the page of sacred history, placed on the ground of truth.

But, although the first man must have been complete in his corporal structure, there can be admitted no conceivable faculty, in simple organization, of reproduction. This faculty, with all the other faculties of animation, must have been superadded by some extraneous power, of which I am hereafter more particularly to speak.

The first opinion which, in the course of our discussion, I have to encounter, is that which states life to be the consequence, or result of organization. There is an incongruity in this opinion, which, from the grossness of its nature, I am surprised could have eluded the most hasty or careless examination. Does not a most glaring absurdity present itself at the threshold of this view? Can it be conceived that bodies could grow, receiving a constant increment of volume; could have parts in regular succession evolved; could possess certain animal attributes without being enlivened?—At what period, or stage of organization, is it conceived that life is produced? Are we to attribute the first stages of growth and evolution, to some imaginary powers of mechanism, or to forces of elective attraction? We have

no respectable ground upon which we can with safety rest the assumption, that the primordial materials, furnished by the act of sexual commerce, are compound or organized bodies. They may be simple, elementary atoms. That the proportions furnished reciprocally by the male and female are enlivened, is obvious. And, although organized, this organization is animal, and, in my opinion, to be explained only on the attributes of life. Where a thing, with all its circumstances is wholly inscrutable, it does not become modest science, too boldly to determine on its condition. No experiments, I believe, have as yet been successfully engaged in, by which the way has been opened to this mysterious subject.

The sameness of structure, and uniformity of organization of the human body, I deduce, as I do the structure and uniformity of figure of the inferiour animals and vegetables, from a fixedness and unchangeableness of laws in the principles of life proper to the living body. It is owing to these laws, it is apprehended, in the principles of life, that the various families of animals and vegetables continue to be separate in their generick distinctions throughout successive generations. It is to these unalterable laws of life, intrinsick to every organ, that the evolution of the several parts and members of the body is to be referred. Were it otherwise, monstrous productions would continually

arise, and the beautiful aspect of nature be deformed by the greatest irregularities and confusion. When life becomes extinct, how rapidly does all this elegant symmetry and beauty of structure dissolve and fall into ruin !

“ Life is” also said to be “ a forced state.” This proposition was, I believe, first advanced by Brown, of Edinburgh, and afterwards advocated by Professor Rush, of Philadelphia.

In the proposition, the first thing to be considered is the striking carelessness and looseness of the style. So unhappy indeed is it, that, to my apprehension, it conveys nothing determinate or intelligible. The words, as they stand related, are circumscribed by no meaning.

To contend, that life was forced on matter by a creating intelligence, surely was not the meaning of the writer. This would be to admit, that inanimate matter has a faculty to resist the exertion of creating energy. Where a condition can be said to be forced on a subject, there is an implied admission, in the assumption itself, of some faculty of repulsion, either physical or intellectual, in the subject, exerted in opposition to the agent. Otherwise the forced state becomes unintelligible. But, I hope, it is scarcely necessary to consume time in attempts to prove that no matter,

much less unenlivened matter, can possess any faculty of resistance against creating power.

But we are told, that "life is a forced state." Shall I be excused, if I give to this arrangement of words a more intelligible form. Or, in other words, put it into English, and say that, "the state of life is the effect or consequence of the application of force"! By such a change, it might be reduced to a form manageable by our understanding. As it now stands, it assuredly is susceptible of no rational interpretation. And I have no hesitation in giving it as an opinion, that neither Brown nor Rush comprehended the import of their own select language. I do not here break in upon the common courtesies of decorum and propriety, consecrated by use, and rendered sacred by time, in questioning whether, or not, the learned writers, above quoted, comprehended their own words. This is not the only passage, in the writings of these celebrated authors, where an excess of splendour has produced darkness. In other words, where a sprightly fancy has so far outstripped a lingering judgment, as to leave the latter totally benighted. For the judgment is not always sufficiently adroit in extricating itself from meshes fabricated by imagination. There is not a doubt that the writers meant something, but, very much to the incommodity of the reader, they expressed nothing. The gentlemen should have furnished a key to their cipher.

To give to the opinion all the advantages to which it can in any way be entitled, let us admit the structure of an English sentence, as already suggested. It shall be, that “the state of life is the consequence of the application of force, or” as Beddoes has it, “the effect of stimulus.” I wish to indulge all liberality and candour. I have no desire to place the sentiment on unfavourable ground. For my purpose, it is of no moment how the sentence stands, or the language runs, I expect to show that the opinion is absurd and unphilosophick.

If “the state of life be the consequence of the application of force”—it must be enquired of what kind of force. Is it mechanical or chemical? Can it be the result of the application of force by the pulley—or by the lever—or by the screw—or by the wedge—or is it deducible from simple gravity? Or shall we refer it to the artful combination of any two, or many of these forces? I am not attached to mechanical forces. Would the powers of chemistry answer our views better? Shall we consider it the playful result of simple or double elective attraction, and that muscular motion is a chemical explosion?

The advocates of the opinion of Brown, are admonished to institute their experiments, by the application of any, or every kind of force on inanimate matter, and observe the result. If it be meant, that this effect of stimulus is

the consequence of aliment, received into certain organs, I would advise alimentary materials to be placed in contact with an unenlivened animal viscus, and the result to be carefully observed.

But, "since life is solely the effect of stimulus," it were not irrelative to propound the question—on what does this stimulus act?—Surely gentlemen cannot mean, a stimulus can act without a subject on which it may exert its agency. Alimential substances placed in contact with lifeless organs, would to those organs not be aliment, but merely dead materials. Therefore I do apprehend, that if aliment produce stimulation, it must be by being applied to something capable of being stimulated—that is, to an organ in an enlivened state. But this would be to abandon the ground, that life is the effect of stimulus solely.

Thus it would appear to minds of the most moderate capacity, that if the enlivened state be necessarily present ; or if the stimulus to be operative, must find the condition of life in the organ to be operated on, such condition cannot possibly, by any rational mode of reasoning, be deducible from the operation of stimulus, or be in any way the effect of stimulus. Hence the conclusion that, as it regards the alimenteriness of any material, it borders on *absurdity* to attempt to maintain, that life is a forced state, or the effect of stimulus.

Moreover, I am disinclined to admit, in the proper and strict sense of the term, that any nutrimental matter, as such, can or does stimulate even a living organ. That the gross substances thrown into a living stomach may stimulate, and that a part also of what is taken up by the lacteals may stimulate, is defensible and true, but I do most strenuously contend, that the excitement or stimulation here, is fairly referrible to the grosser, extraneous, and heterogeneous materials that accompany the more purely alimentary substances. To these less refined admixtures, to be ejected as recrementitious matter, under processes of secretion by appropriate emunctories, I say, may be safely ascribed the excitation, while whatever can be of durable tone or strength, is deducible from the living powers acting on the really nutritious portions. It cannot be admitted, that articles of pure alimentation ever do stimulate—To stimulate, is offensively to disturb; and that which disturbs, acts unnaturally—disordering the organ to which it is applied.

The alimentary appetites of the stomach are to be referred, not to the foreign stimulation of food, but to fixed and intrinsic laws proper to the stomach itself—Like the sexual desires and passions, they grow out of the natural condition and economy of the organ.

Surely the fluid furnished by the stomach, as to its nature and properties, cannot by any rational physiologist be attributed to the provocation of aliment taken into this organ.—Nor do I apprehend, that the process of assimilation can be deduced from the stimulation of chyle circulating through the body. It would be more probable that this elaborating operation, by which inanimate matter is refined and transmuted into animated organization, should be laid to the account of powers and agencies inherent in the law of the operating instruments. Are we never hungry except when our stomach is filled with food, and subjected to excitation therefrom ?

That a given temperature is requisite for the existence of animated bodies, is that against which I am not inclined to contend—or that air is necessary to the function of respiration. But because heat constitutes a necessary condition for the continuance of life, and that air is indispensable to the operation of the respiratory organs, we are not justified in the deduction, that life is the *result* of force, or the *effect* of stimulus. A difference may be perceived between the *continuance* and the *existence* of life—between a thing as to its absolute reality, and an associated condition of its prosperity.

The temperature of the human body, I conceive, is to be ascribed to animal powers, and

not chemical actions. If it depended on chemical laws, when the living and healthy body should be surrounded by a temperature of one hundred and fifty, the heat of the circulating blood would rise up to the standard of the medium in which the body might be immersed ; or if the medium were reduced to thirty degrees, the temperature of the circulating mass, at its source of circulation, would be depressed to that point. For the degrees of heat would be regulated by the invariable and known chemical laws and affinities of heat. But certainly no philosopher, who regards his reputation, would contend that the surrounding medium would at all regulate the temperature of the blood in the heart of a living and healthy body. Yet were the body lifeless, it is very obvious that its temperature would be graduated by the condition of the surrounding medium. Hence we may infer it to be indisputable, that the temperature of a healthy body is ascribable to laws of animality, and not to any play of chemical affinities or agencies.

So far, indeed, is the body from deriving its life from the stimulus of heat, the living body in fact, and to a given degree, does by its principles of animation hold dominion over, and control the chemical laws of heat. If it were otherwise, the healthy body could not be fixed in its temperature at ninety-eight, nor at any other invariable point.

Not only may the fixed temperature of the living body be referred to the principles of animation, but I do not think that there would be much danger of error, were we to attribute the function of respiration, together with all the changes of blood consequent upon respiration, to the same source.

Whether the blood receive from without, or lose from its constituent parts, is at present of little moment ; or whether the blood both receives and gives out reciprocally, which I admit to be the fact. Whatever may take place is deducible from living agency. The materials that are carried into the system, I insist, can be introduced only by the absorbents acting under laws of their own economy. And it may moreover be contended, that there can be nothing like a property of stimulation in the materials, nor an action in any degree approaching to stimulation, between the material to be taken in and the living instruments. Every conceivable attribute or condition, beyond a mere suitableness for the purposes of the animal economy, in the substance to be operated on, whether this be oxygen, or phos-oxygen, or whatever it may be, a guarded science rejects.

The same may be said of the materials to be eliminated, whether carbon, or hydrogen, or any thing else. Those materials must be separated from the blood by exhalants, living

agents, acting under the regulation of the principles of animation, inherent and proper to themselves.

To ascribe the redness of the blood, or any other change consequent upon respiration, to chemical laws, is to abandon the first dictates of common prudence, and common sense. I have nothing to say as to the probability or certainty of chemical operations, after the materials may be brought into close union with each other, either in the blood or in the air-cells. But that oxygen, or phos-oxygen, or any other material is taken into the blood through the living membranes, by the chemical affinity acting between this material and any property in the blood of the lungs ; or that the oxygen of the air in the lungs, can by any chemical forces elicit or attract the hydrogen or carbon from the blood, through the tunicks of the vessels and common investing membranes, I hold to be among the grossest absurdities of modern chemistry.

If the oxygen and carbon have affinities for each other, a proposition not to be denied, these affinities must be equal and mutually operative. Then upon what ground do chemists contend, that the carbon of the blood can decompose the atmosphere, and attract the oxygen through the membranes to itself ; or, on the other hand, do not chemical philosophers take the utmost liberty with our

understanding, when they have the indiscretion to assume, as a postulate, that the oxygen of the air can and does decompose the blood, eliciting by chemical force and violence the carbon through the membranes ?

The whole of the assumptions reposes on the begged question, and this, in its turn, rests on a most gross misconception. For whatever forces there may be, must be equal and reciprocal, and any chemical union that might take place, forming carbonick acid gas, or any other compound, would of necessity occur in the central point, between the air and the blood ; that is in the walls between the two agents, and disease be the necessary result.

How have chemists ascertained, that the chemical forces between the oxygen and the carbon, are stronger than what may exist between the carbon and some other constituent of the blood ?

But it is contended, that if blood be suspended in common air, invested in a bladder, or any similar membranous bag, it will assume a red vermillion colour. And that this cannot be considered as being owing to any animal powers : This must be laid to chemical account. It is granted.

And, illustrative of this chemical fact, if any quantity of blood be suspended in a bladder

in a medium of two hundred degrees, or of thirty degrees, the blood will be of the same temperature of the medium in which it may be suspended. And it is so necessarily by chemical laws. But does this illustration of the chemical capacities of blood for heat, when enveloped in a dead membrane, and under circumstances where chemical affinities can have their play, demonstrate that the fixed temperature of ninety-eight degrees in the living body, is to be explained on the same chemical principles of dead matter? It can only prove the affinities between some parts of the blood and the air, or some constituent of the air.

If living membranes can control the influence of so subtile and penetrating an element as heat, and one of such extended chemical relations and force, and can remain impervious to its strong powers, surely we may be excused in admitting that the same living structure may serve as a barrier against the grosser material of oxygen or carbon, especially as, by admitting animal powers, a demand not extraordinary in a living body, we can more easily and rationally explain the phenomena consequent on respiration.

Although the blood in a bladder may assume the vermillion colour, is it certain that the colour assumed is exactly the colour of the circulating blood in the living arteries—that it is neither lighter nor deeper in its hue? It is

also asserted, that the blood in a dead membrane not only assumes the vermilion colour, but that it is heavier. Is this the case with the circulating blood? and how does it square with the chemical assumption, that carbon is given out from the blood?*

Chemical agencies are not to be admitted in animal function.—For as is the agency, so must be the result. And if chemical agency be admissible in one, why not in every animal function, and the whole animal system be conceded to be a complex chemical machinery. The means must be as the ends. Against the air in the bronchial cells being decomposed and appropriated to animal purposes by chemical means, I do most strenuously contend; nor can I accede to the proposition, that any matter, gaseous or liquid, can be thrown off from the blood otherwise than by the immediate instruments of life. On the contrary, I assume that whatever may be taken into the body in the acts of respiration, is introduced by living tubes, acting under laws of life proper to themselves; and whatever may be thrown off, is eliminated by vessels of secretion or exhalation, regulated by attributes suited to their own economy; and that the living system operates either immediately by the nerves, or immediately by its absorbents or exhalants; and that in neither case are the living agents acted on, by any real or supposititious stimulus.

* See Johnson's Animal Chemistry.

Whatever stimulates disturbs, and whatever disturbs throws nature off the scheme of her proper economy.

But it may be advanced, and I believe has been asserted, that the functions of the body may obey chemical laws, under the dominion of the principles of animal life.

A law in nature is a rule of action, and we apprehend that no philosopher, except such as abandons the suggestions of a rational logick, will contend, that any physical operation can go on in obedience to rules or principles, fundamentally various and distinct. It is hoped we need not add, that the principles by which enlivened matter conducts its processes, are distinct in kind from chemical affinities.

If respiration be an animal function, as has been remarked, in whole, it must be so in all its parts—for as are the parts, so must be the whole. And to speak of chemical influence, in that in which it were absurd to conceive the presence of a chemical rule or law, to me, is unintelligible. Contraries must equally oppose each other, and rest or death be the result.

“The *definition* of life is to be sought for in abstract considerations; it must be found, I believe, in the general *perception*; life is the totality of those functions which resist death,” says Xavier Bichat.

It is scarcely possible that words, with the utmost care, and most cautious address, could have been arranged into order according to the legitimate rules of grammar, more completely unsusceptible of all meaning than the phraseology of the paragraph above quoted. There cannot be adduced from the whole extent of physiological literature, a proposition more indefinite or unintelligible.

A mathematician, when addressing himself to a person conversant with the objects of the mathematical science, would be comprehended, were he to discourse on the *definition* of a triangle, or a circle, or a square. But when a Physiologist refers us to a *definition* of life, and adds, that it is to be sought for in abstract considerations, and that these abstract considerations are a general *perception* ; he surely means to amuse our auditory organs by the melody of sound without sentiment, or to divert our visual capacities by a display of words and sentences without sense or ideas. That life, as to its reality, is a legitimate object for synthetick reasoning founded on a consideration of the sensible phenomena of life, is a rational proposition against which I am indisposed to contend. But, how life, in its nature and essence, can be a totality of the functions which resist death, is what I do not comprehend. A totality of functions is a sum or totality of exertions, and surely the exertion of a principle and principle itself must be somewhat distinct

both as to reality and as to time ; a principle may exist without being employed in a sensible function.

But admitting for a moment, against which every rational mind will forever exert its powers, that life and the totality of its functions are essentially the same. In what manner can this totality of functions be tortured into a *general perception* ? A totality of functions in an organized body may be an aggregate of physical operations. Yet, I do not see how this sum of physical functions can be understood to be a general operation of mind, which all men of understanding admit a *general perception* to be. A totality, or aggregate sum, of physical functions may under appropriate circumstances give occasion to perceptions, rapid and various, and bearing an intimate mutual relation, from certain laws of association, and may constitute a kind of general or rather compound *object*. But, there is no way by which a set of physical functions can be construed into operations of mind. And still more difficult is it to conceive, how intellectual operations can become the principle of animal or vegetable life, upon which breathing and circulation, and digestion, together with the various secretions depend.

“ Such is life, says Bichat, in the whole ; examined more minutely, it offers to our view two remarkable *modifications*, the one is *common*

to vegetable and animal, the other is peculiar to the last."

That all organized and living bodies, animal or vegetable, must possess a principle of life, is obvious. But so far from the life of an animal and that of a vegetable being *modifications* of the same principle; it is incontrovertibly clear, that between the various classes of animals themselves, such as quadrupeds, birds, and fishes, there must be a fundamental and radical difference, otherwise the uniform dissimilarity of structure which constitutes the unvarying difference of class or assortment, or the uniform dissimilarity of structure which establishes the immutable difference between the various kinds of the several classes, would be no longer fixed and determinate, but contingent and alterable.

I should be gratified to know upon what facts or reasoning it is that M. Bichat has founded his proposition, that the difference between the life of the animal, and the life of the vegetable, is no more than separate *modifications* of the same principle: Is it because the animal and the vegetable are both organized bodies? This proves nothing as to sameness or identity of life. It only suggests, according to similarity of structure, wherever such can be found, which I believe remains to be discovered, an approximation of laws.

When enlightened philosophy contemplates the organization of a man or any other animal, and that of a tree, it is, I apprehend, very much disinclined to conclude the two to be but *modifications* of each other. In its most drowsy moments, I am induced to believe it would readily determine them to be radically and fundamentally distinct.

Moreover, M. Bichat suggests that “each of the two lives, animal and organick, is made up of two orders of functions, which succeed, and are connected to, each other in an *inverse ratio*.”

That “it may be said that the vegetable is the rough sketch, the *canvass* of the animal; and that to form this last, nothing more is necessary than to display upon this canvass the external organs proper to establish its different relations”—

That “the eunuch possesses *less* vital energy; but the phenomena of life in him, are developed in *greater* plenitude”—

That “the nutritive particles alternately absorbed and ejected, pass from the *animal* to the *plant*, thence to the *brute*, and so on again to the *animal*.”*

* Translation by Dr. Watkins.

The whole of the above are beyond the range of my intellect ; the reader may possibly decipher them.

From the incongruity of the opinions and arguments of M. Bichat, if concatenated peu-rilities may hold the rank of arguments, we may be permitted to discontinue our examination of his doctrines of life, and conclude from the sum of what has been said, that life is not the consequence of organization, nor the result of force, nor the mere effect of stimulus, but that it is a condition impressed on matter, or superadded to it by creative energy—and farther, that we have no good reason to believe that animal and vegetable organizations are dependant on the same principle, differing in gradation only.

In a converse, between male and female, materials are furnished possessed of living principles, and these principles are endued with such laws of mutual association as are conducive to, and efficient in the constitution of an organized entity, which we term an embryo. That the organization of the embryo is dependent on the attributes of life, with which each elementary atom is provided and endued, we assume from the uniformity of structure in each assortment of animals, and from the similitude of figure and expression of countenance discoverable between the offspring, and the male and female parent ; which similitude is the result of the

operation of the laws of life, under the dominion and regulation of which, the organization is constructed. Those laws of life, I apprehend, are general and particular : general as regards the compound whole, and particular as relates to the evolution and function of each individual organ and member.

This growth or accretive process, the result of the living powers acting on nutrimental matter, and transmuting it into animated organization, is to be classed among the early phenomena of life. It is, indeed, palpable and sensible in its physical condition, but not in its manner. That the organization is referrible to attributes of life may be argued from the important fact, that immediately on the cessation of life, there is a disarray and dissolution of parts. Were the organization the effect of chymical laws, this would not necessarily and uniformly be the fact ; the dissolution would not be consequential on death, except life be a chymical principle.

If the original constituent atoms were not intrinsically enlivened, the corporal whole, whether of the adult or embryo, would only be an aggregate of lifeless materials ; at best a chymical compound. For the *whole*, in fundamental attributes and principles, could only be according to the essential nature of the *parts*. “ *Ex nihilo nihil quicquam fit,*”—From nihility proceeds nothing.

This growth, or accretion, appears to be a species of assimilation, or working up of the alimential matter by a peculiar apparatus, operating under fixed and determinate laws. The apparatus, or immediate instruments concerned in this accretion, or function of assimilation, in all probability, are chiefly the finer arteries, under the control of the nerves, acting on the blood which has been fabricated and prepared by the stomach, the lungs, and the circulating powers, aided in no slight degree by the absorbent system, and, shall I say, in part enlivened?—Is not digestion wholly an animal process?—Could chyle be made out of the living body? I at present do not wish to extend my views on growth, farther than as it is a phenomenon of animation.

SECRETION.

Another obvious phenomenon of life is secretion. When speaking determinately of secretion, I wish it to be understood as having regard to the separation of the recrementitious materials from the blood by various organs, such as the lacrymal gland; the breasts; the liver; the kidneys; the skin, &c.—and which are eliminated by numerous emunctories. In this more usual and proper sense, the term secretion cannot be strictly used in reference to unborn Infants. For although some fluids, considered excrementitious, are prepared before birth for excretion, they are not thrown

off ; and others are not fabricated until the more advanced period of adultness. At which period we perceive all the various secretions coming into full operation. Each by an appropriate organ ; and this organ acting under the dominion of laws proper to itself. The bile is not secreted by the lacrymal gland, nor the tears by the liver.

In these various operations of digestion ; assimilation ; growth ; and secretion, I am clearly of opinion that the living powers are to be understood as acting on the aliment—the chyle—and the blood, and not the nutrimental materials acting on the living instruments.

EXCITABILITY.

A third sensible expression of life, is the capacity in a muscular fibre, to be disturbed and changed in the mutual relation of its parts : in other words, to be forced by foreign agency into an abbreviation of its length. This capacity is styled excitability, or the condition in a muscular fibre to be acted on.

Upon the authority of Dr. Brown, we are informed that “ excitability,” the capacity in a living muscle to be acted on, “ is the principle on which the phenomena of life depend.” And in page 266 he insists that death closes the labours of life, either from a complete exhaustion, or *extreme abundance of excitability*.—

Thus we are told by this medical philosopher, that *excitability* is the principle on which the phenomena of life depend, and yet that death closes the labours of our lives by an extreme *abundance of excitability* !

If it be admitted, that the phenomena of life depend on excitability. In the direct ratio of the abundance of this excitability must, to all intelligent minds, be the augmentation of the power or force of the living phenomena—except it can be demonstrated, that the abundance of life is the fullness of death.

I admit excitability to be one of the phenomena of life, and that it is always to a given degree in the inverse ratio of the power of life or the faculty to perform. That is, it is always in the direct proportion to the inability to resist. Or, as Brown has it, a super-abundance of excitability is great exhaustion.

“ A super-abundance of excitability is direct debility, and an extreme abundance is death,” and yet excitability is the principle on which the phenomena of life depend! Digestion; assimilation; respiration; the circulation of the blood; in short, all muscular motion are phenomena of life. Are these the functions of the principle of excitability?—And are they in vigour and efficiency in proportion to the accumulation of this principle?

Excitability, or the susceptibility of impression in a muscular fibre, is a kind of negative interpretation of living power. It indeed, rather furnishes an occasion for a phenomenon of life. Perhaps, it cannot strictly be viewed as a direct living expression in itself, but I shall, for the sake of argument, continue to speak of it as a positive quality, thereby granting to those whose sentiments I impugn all the advantages that can arise to them from their own select language.

Yet under every view that can be taken of this susceptibility or liability to impression in a muscular fibre, it cannot, without permitting ourselves to be led into the grossest incongruities, be admitted to be more than a condition or quality of life. It uniformly augments in proportion as the faculty in a muscle to resist foreign impression may be exhausted, and diminishes as this faculty augments.

In proportion as the body recedes from health, from necessity it falls off in its powers and faculties, such as those of growth and assimilation, which surely are phenomena of life, and those by which the vigour or force of life may be measured, yet its excitability or liability to be acted on, will be as the depression of its faculties or powers. Of consequence, without surrendering all claims to sanity of intellect, we cannot accede to the proposition, that excitability is the principle on which the phē-

nomena of life depend, as has been urged by Dr. Brown and others, equally careless and unhappy in their reasoning.

If any set of muscles, for instance those of the arm, be placed in state of desuetude ; be laid out of use for any given time, and then be called on to engage in efforts to which they had been previously accustomed, they would be found to be impotent ; would be distressed and pained on trial and remain exhausted and disabled for some time after. Under such experiment continued for some time, the muscles are pained and disabled from exhaustion, and an encrease of excitability. They are also somewhat attenuated.

The whole of this we attribute to a depression of the powers of life ; a diminution of its quantity. For the faculty to act, or the power to resist or endure being as the quantity of life, so the excitability or liability to be disturbed, must be in the inverse ratio ; that is as the diminution of life.

Not only will the muscles be weak and excitable, inadequate to endure exertion, but they will be diminished in volume. The energies of life in any part will fall away as the living principle shall cease to be supplied to that part by the general laws of the constitution. And thus the more extreme and delicate texture of the superficies, or finer structure of the

internal cells will be gradually yielding and falling into dilapidation and death. They will be dissolved, absorbed, and eliminated from the body as recrementitious matter. It is upon this principle, and explicable on this principle only, that a man who has been without food for any considerable length of time, becomes overwhelmed ; is perhaps killed, by a full meal.

The whole body, in this instance, is seriously affected and becomes incompetent to its ordinary exertions and supplies, but in an especial manner the stomach is oppressed. The whole function of managing and preparing the aliment for the general body is to be executed by this organ, now depressed and lowered in its life. And like any other organ, it succumbs and brings into ruin with itself all the pillars of the constitution.

But were excitability the principle on which the phenomena of life depend, and, in this state of famine, exceedingly accumulated in the stomach, digestion, being the ordinary and natural function of this organ, would more readily, and with less demands on the general body be performed. The same kind of reasoning holds good in regard to all organs in a state of increased excitability.

Sound and enlightened science, must surely refer all this accumulation of excitability and

inadequacy to resist, to a diminished quantity, or less elevated tone of life. None but a mind, seeking for its sources and analogies of reasoning, beyond the confines allotted by common consent to a sound understanding, could possibly deduce the above from an increased sum of the principle which animates enlivened matter.

Dr. Brown has contended that a given sum of this excitability is allotted to every human being ; that it is equally diffused throughout every part of the animal system ; that it is acted on by all stimuli ; and that each operates as may be its intensity.

Were this doctrine at all defensible, every human being would, of necessity, from the moment it received life, be gradually approaching, by a regularly graduated loss of the living principle, to death. As the stimuli would, and Brown asserts do, act equally upon the whole sum of excitability, wherever placed. The whole sum being equally acted on, and equally exhausted, there could be no possibility of a revival or recovery, inasmuch as each source would be laid under equally wasteful contribution. A man could be a being of but a few hours or days at most. Not only so, but a child would be possessed of higher animal powers than a man ; for the nearer to the inconception of life, the less would be the exhaustion of the total sum.

When Dr. Brown and his warm admirers, contended for his excitability being equally diffused through every part of the animal system, they could not certainly have intended to convey the idea of its being equally possessed by the bones, tendons, muscles, membranes, &c. These surely are not alike excitable.—We will grant they meant an equal diffusion through what are generally denominated the *vital*; *animal*; and *natural powers*. That is, the powers or instruments of assimilation, muscular motion, and digestion.

Under this most favourable aspect, what could be more arrogant and puerile than that philosophy which would insinuate to a rational mind, that the whole sum of the principle of life, as Brown considered it, was during profound sleep in a high degree of exhaustion!! An animal while asleep ceases to move; to feel; to perceive: but does the stomach cease to digest, the lungs to breathe, or the heart to beat?

The laws of sleep, like those of vigilance, are deeply laid in the primordial principles of the animal constitution. But they are, in operation, restricted to the instruments of locomotion and perception.

All animals after a full meal are induced to sleep, and this by the laws of the constitution. What evidence can be furnished, that the sto-

mach is less prepared to manage the aliment committed to it ; or the heart more incompetent, efficiently, for the purposes of the body, to contract and dilate ; or the lungs to sustain their relation to the air, during sleep than vigilance ?

If the stomach, and heart, and lungs, and brain were equally exhausted and quiescent with the instruments of perception and locomotion, would the animal ever awake from a profound sleep ? Whence would be the supply of the principle of animation. The first night would end the life of man. But fortunate for our race, and unfortunate for whim and hypothesis the stomach never sleeps, the heart is never weary, and the lungs never rest, more in sleep than in vigilance.

MUSCULAR MOTION.

There are certain functions proper to animality, which are strictly said to be performed by muscular agency, distinct and separate from the ordinary operations of secretion, growth, &c. Such are the acts of respiration ; deglutition ; locomotion ; and circulation.

Those acts are supposed to derive their origin from laws of the animal constitution, and are necessary to its existence. So soon as a child is born we perceive it to breathe, swallow, and move its limbs, although awkwardly

and without determinate object. We cannot with reason attribute those various muscular operations to will or intention ; nor can we refer them to the agency of a foreign stimulation ; they are of the constitution, and instinctive.

When a child breathes, to say that the muscles subservient to respiration are stimulated, or that the nerves by which those muscles are controled, are roused into action by the stimulus of the air, were to afford an occasion of ridicule, or to provoke contempt. We perceive and acknowledge those respiratory acts to be a part of the necessary and vital operations ; that they are of the constitution, and immediately dependent on the laws of the organ concerned, the lungs, and certain associations which this organ has in relation to the general living powers of the body. To ascribe the acts of respiration to any stimulus, acting directly on the muscles or nerves concerned in respiration, were to refer them to contingency, and place them without the established and uniform laws of the system.

The regular motion of the respiratory muscles is as much a part of the animal economy, and as immediately under its control and dominion, as the original construction of the muscles themselves, the nerves that govern them, or the bones they act on.

No physiologist would say we breathe because our muscles are stimulated, but because respiration is necessary to, and a constituent part of the living system, and arises out of the general sympathies and necessities of life.

What particular ends may be accomplished by the function of respiration, is not in any degree an object of my present disquisition, I merely wish to express it to be an operation directly dependant on the laws of the constitution.

What I have said of respiration may, with a little qualification, be extended as to acts of deglutition. Whenever we swallow, by a complex agency of the muscles of the throat, the wind-pipe is elevated, and the os hyoides brought nearer to the tongue, so that the epiglottis may be well applied to the glottis, and the morsel or fluid to be swallowed, prevented from passing into the lungs. This operation, as complicated as it may be, is ordered by a singular and inscrutable association of laws intrinsic to the muscles concerned, and is not in any wise ascribable to any stimulation of any foreign power. If it were a thing of contingent stimulus, it would eventuate in irregularity.— But it is uniform, whether our saliva is to be conveyed to the stomach, or a glass of beverage is to be swallowed. Nor is it a thing of the will, farther than the act of deglutition is determined on ; it is rather involved in the gene-

ral scheme of animal sustentation ; and is of the constitution, under the superintendence of immutable laws.

But let the above functions be, as much as possible, sustained by associated relations between the particular muscles serving as immediate instruments, and the general powers of life, physiologists insist that the action of the heart is the result of immediate and direct stimulation. But why the muscular fibres constituting the walls of the heart should be necessarily provoked into contraction by an offensive quality in the blood, I cannot conceive, more than the fibres which make up the muscular instruments of respiration and deglutition. It is contended, that a certain material is received into the blood through the lungs, and that this exotick agent becomes a source of provocation to the heart.

Were this the case, the auricle and ventricle would be more especially stimulated, as the agent would be more immediately applied to them, than the right auricle and ventricle to which it can be but ultimately applied. But the right side of the heart acts with equal vigour, in proportion to the thickness of its walls, as the left.

Again, if the blood returning from the lungs brought an agent of stimulation, how is it that the fibres of the auricle, which is in contact

with the blood thus charged before the auricle is filled, do not contract? The same question may be propounded as to the fibres of the left ventricle, which are first acted on by the blood rushing into that ventricle? We do not know that the excitability of the heart is to be operated on, only when the ventricle is full. I do suppose, that the first fasciculus of fibres coming into contact with the blood, would be acted on—And, if so, the contraction would be irregular, and the regularity of the circulation disturbed, and the whole economy thrown into disarray.

It is to my mind, a little inconceivable, how an organ, the heart, should be the chief instrument in the fabrication of a fluid, and yet that fluid not to be, in its qualities, exactly consonant to the nature and attributes of the organ that made it. If the organ be a part of the living system, and its function be to prepare a fluid for the benefit of itself, and the general body; either it is imperfect in its operation, or the fluid must be suited to the object for which it is intended. To suppose it imperfect in regard to the end for which it is intended, were to forget all proper regard to our common sense.

I can as little suppose, that the blood is in any of its properties offensive to the living powers of the heart, as that the tears or milk should be irritating to the organs that secreted them.

An organ must make whatever is the result of its operation conformably to its own laws, or it is diseased and imperfect, and the constitution to which it belongs, must quickly fall into ruin.

Why the auricles and ventricles of the heart should not contract and relax by their own laws, under associated relation to the general living system, as the intercostal muscles, diaphragm, and muscles of deglutition do, I cannot imagine. It is admitted on all hands, that the muscles which elevate the throat, in acts of deglutition, are moved in obedience to laws of sympathy between them and the palate ; that the intercostals and diaphragm expand the thoracick cavities, by associated relations with the laws of life in the lungs, without stimulus from the morsel or the presence of air ; and yet, it is strenuously asserted, that the heart must be irritated by oxygen, or something else ; which oxygen, or something else, in fact, has never been ascertained to be taken into the blood. So much are some minds bent up to the defence of what they have heard, but never understood, and cannot comprehend !

It should at least be ascertained, that something is, in reality, received into the blood through the instrumentality of the lungs, before we enlist ourselves in defence of any particular application of this supposititious material. There may be something received, or

there may not be something received. And, even if there be, it may be destined for some other purpose than stimulating the heart ; at least it must go its round through the absorbents.

For I do apprehend, from the general analogies of the animal body, that the lungs, like the lacteals, act in accordance with each part, and the whole system. Whatever they may receive, is taken in by laws of sympathy and relation, and that they do not, for the purposes of the economy, admit that which is irritating to any one part. For if offensive to one part, the material received may be so to many, or to all, and hence would arise confusion and ruin. That much is admitted, not nutrimental, is very true, but this is separated and eliminated by proper emunctories, and not elaborated into a nutritious article, to be appropriated to the purposes of animality, by an organ whose business, it appears, is to prepare for the living system, and the support of the whole enlivened fabrick. Whatever stimulates or excites, is medicinal or poisonous.

In my opinion, the heart acts by laws intrinsic to itself, without provocation, and in perfect obedience to fixed and determinate relations with the living whole. It would be quite as tenable to advance, that all the exhalation and absorption of the animal body go on under irritation, as that the heart acts from

irritation. For the one order of operations is not more natural or necessary than the other ; they are all within the general scheme, and, I do presume, equally under the fixed provisions of the system, and all acting to determinate ends.

Muscular motion, as a living phenomenon, is discoverable in other operations than breathing, deglutition, circulation. In those muscular efforts by which the body is transported from one place to another, are manifested also the expressions of animation. But, while those of which I have been speaking are necessary and vital, these now under consideration are voluntary and expedient. The one set is kept in ceaseless operation by the urgent necessities of the body—the other called on at will : the one is immediately necessary to life—the other mediately—and both by laws inscrutable.—Our heart beats without our consent, but we will our acts of progression.

The muscular exertions by which heavy weights are raised and projected ; the achievements of the gladiator and boxer, together with all other gymnastick exercises, are referrible to the faculties of life resident in the muscular fibre, and are uniformly in the inverse ratio of the excitability, or impatience of the muscle to foreign impression. To what I have said I might add the fact, that not unfrequently, by muscular exertion, tendons are

lacerated, as has been the case of the Tendo Achillis ; and there are not wanting instances of bones being fractured by muscular energy : this frequently happens to the patella.

The discrimination between animal and vegetable life, has no relation to my present design ; nor were it prudent that it should have.

SLEEP.

Another phenomenon of animation is sleep ; and much has been said of late, to show that this condition is a partial disease ; that it is the result of casual stimuli acting on the living excitability, or, in other words, on the capacity of the body to be impressed. I will attend for a moment or two to this subject.

“ As death” says Brown, “ closes all the labours (physical operations) of life, so sleep closes those of every day.” Than this sentence, as pretty and fluent as it may be, there are few in scientifick literature, more unintelligible or absurd. That “ death closes all the labours of life,” or, in other words, that the cessation of all living operations is death, is a matter very readily comprehended. But what are we to understand, by sleep closing those of every day ? It is obvious, that the vital labours of the day are not the absolute and identick labours of the night. The operations of the night must be the operations of the

night. Then, what vital labours of the day are closed by the sleep of the night? Every man of sound intellect knows, that the circulation of the blood goes on at night as efficiently for the purposes of the animal economy, as in the day. The same is a fact with respect to digestion; assimilation; breathing; growth, &c. The analogy or parallel between death and sleep, as attempted by the ingenious writer just quoted, is forced and ridiculous. That our social and moral labours are closed by sleep, and that our senses cease to be used as instruments, both in relation to the external material world, and as means of acquiring gratification or support to the animal machine, is very true. But surely all the physical labours of the body are not closed at night, by sleep or otherwise. Nor does it appear, to my understanding, that the labours of the night are less vigorous, or less vital than those of the day. It is very probable, and might almost be advanced as a fact, that digestion and assimilation go on more *perfectly* during sleep, than during vigilance; and that the body acquires more *living organization* during the former than the latter state.

It is well known, that all animals, man not excepted, when they have been indulged with a full meal, retire to rest and to sleep; and that they awake refreshed and invigorated. Were sleep a partial exhaustion of excitability, or that principle, according to Brown,

upon which the phenomena of life depend, as should be its profoundness, would certainly be the vital exhaustion or approximation to death, of consequence, the positive disability of the body. And hence must necessarily follow, not invigoration, but ruin and inexistence. Especially were this excitability equally diffused throughout the living whole, and equally exhausted as some learned gentlemen contend.

The more complete and profound our sleep is, the more refreshed and invigorated are our bodies upon being awakened: According to the Brunonian philosophy, the nearer the body in its condition approaches to dissolution, the more elastick and vivacious it becomes !!

That sleep succeeds to fatigue or exertion, is by no means an evidence that it is the *immediate consequence* of this fatigue or this exertion. The fatigue and effort, by calling strongly on the resources of the constitution, put the body into a condition requiring invigoration and support; and the laws of sleep, closely interwoven with the fundamental principles of life, come into operation for the restoration of the vital powers, on principles intrinsick to the animal economy. In illustration of this, if the fatigue be very considerable, the economy of the body is not unfrequently so disturbed as for general laws of health to be interfered with, and then the body remains restless and fatigued, the powers of sleep, a part of the laws

of the economy, being unsettled and forced into disarray.

We sometimes cannot sustain a state of watchfulness, because the body is disturbed and exhausted in its constitutional powers. We cannot at intervals derive the ordinary refreshment from sleep, we even are unable to procure sleep—because the animal resources, upon which depend health, and all the functions of health, are disordered and intrenched on by excessive fatigue or exertion, mental or corporeal.

It would appear that the state of sleep is not less a state of nature and of health, than the condition of vigilance. They are, so far as the economy of the animal body can be looked into, equally natural and alike deeply laid in the foundations of life. That of vigilance serving more particularly as an occasion for social and moral exercises, together with the procurement of the means of animal sustentation. That of sleep, more especially for the private affairs of the animal economy itself. When the inferior animals are to be fatted, they are confined in retired and dark habitations, that they may sleep. And, in proportion as they may be without motion and enjoy sleep, will be the rapidity with which an increase of bulk is acquired. And, I believe, the less they are molested and urged into ex-

ertion, the stronger the proclivity to semnolency, and acquisition of corpulency.

I would also argue, that sleep is radicated in the common, healthy laws of the body, from animals in the general, of the same family, sleeping nearly an equal portion of time. And, I conceive myself to be strongly fortified in this opinion from the fact, that sleep is the primary and original condition of the animal body. If a child be born at seven months or eight months from conception, it will sleep much more than if it were produced at the usual period. And this will always be in the ratio of its immaturity. Hence, may be deduced the high probability, if not absolute certainty, that the condition of sleep is antecedent to the state of vigilance; and, of necessary consequence, it would involve the grossest absurdity to assert, that sleep is the consequence of stimuli acting on a state of vigilance. For this were to make the antecedent consequent upon the subsequent.

Sleep affects and extends its powers to our senses and faculties of voluntary motion only. And, hence it is, that our minds appear to sleep; but, this is an illusion. Our minds appear to sleep, only because the medium through which the phenomena of mind are expressed, is rendered, by sleep, unsuitable to the exertion of mental operations in regard to what is without. Our minds may act and be acted on without

the immediate employment of our senses. This is exemplified by dreams and intuitive knowledge or prophecies. That all our knowledge must first be in our senses before it is in our intellect, is the speculation of heathenism, and not to be recognised and defended by enlightened science. It is in opposition to all the fundamental principles of christianity and the experience of life, upon which I have no hesitation to bottom what I may say on operations of mind.

Some animals devote seven or eight hours in the twenty-four to sleep, others consume a longer or shorter time, while particular families of animals enter annually into a hibernal state. Is the ordinary state of the vegetable world, during winter, a condition of vegetable sleep ?

If sleep be a disease, a graduated stage to death, would not the body be improved in its health and vigour, by such measures as would altogether keep it from a state of somnolency ? The more completely the body is preserved in full exemption from disease, the vital powers are, for the most part, augmented and invigorated ; then surely a continued state of vigilance would be desirable to every animal.—Are the friends of a partially exhausted excitability, as a state necessary to sleep, prepared to encounter these consequences, inevitable and certain, of their favourite speculation ?

Exertion and fatigue, or the operation of stimuli may, in many instances, give occasion for sleep to take place, but cannot be viewed as the intrinsic and absolute causes of it, any more than the light of the day can be considered as the cause of the darkness of the night : or the centripetal be admitted as the source of the centrifugal motion of the heavenly bodies.

Those different conditions and revolutions arise out of the established order of nature, and are only so many varied expressions of her general and compound economy.

All artificial means which, in the hands of the healing art, are used to produce sleep, should rather be considered as composing the disturbed conditions of the body, and as indirectly giving *occasion* to sleep. By lessening pain and morbid irritability, they permit the body to fall more especially under its natural and healthy laws. So far as anodynes otherwise act, they should be rather viewed as poisons, producing ease and tranquillity by immediately affecting the principles of animation, and from such conditions of stupor the body never is revived with sensations of refreshment and invigoration, but always with a sense of increased languor and weariness.

VIGILANCE.

The state distinguished from that of rest and sleep, is the condition of vigilance. While treating of sleep, I have of necessity been obliged frequently to advert to the opposite condition. What we mean by watchfulness, is that peculiar preparedness in the instruments of perception which enables the mind to hold intellectual intercourse with the external world ; to which may be added the state of ready co-operation in the muscles of voluntary motion, to procure and prepare the materials necessary to animal comfort and existence.

Without the powers of vigilance, man could acquire no knowledge of his fellow man, or of the material universe, nor be able to seek out his necessary aliment. And it is obvious, that those instruments alone, which are called on in social intercourse, and acts of procuring physical support, are subject to sleep. The powers immediately concerned in the appropriation of the alimentary materials to the private uses of the body, are always active, and ceaselessly perform their unwearied functions.

These are the phenomena of animation.—By what has been detailed, animal life is ascertained to exist, but what it essentially is, re-

mains inscrutable to the most industrious effort and keenest investigation.

Having discoursed so far on the sensible expressions of animal life, my next examination will be in regard to its seat, or the subject in which it may reside, in other words, its ubiety or whereness.

FINIS.









JUL 15 1949

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